

Columbus O. 9 February 1850

Dear Sir

Since long time, I was desirous to write you, but I was afraid to disturb you, for I know how precious is your time. But now, you have given me yourself a good reason to be excusable of this letter, for, a few days ago, I received by Mr. Sullivant with the greatest pleasure the second volume of your magnificent Genera. Certainly, I have done nothing to merit such a benevolence and friendship as you have given me so many proofs since I am in America; but it is a reason to be more and more thankful of your kindness. Your Genera and particularly the second volume is the most beautiful and valuable book I ever saw in Botany. Moreover, I read your works, now I admire the correctness of your descriptions, the rectitude of your affirmation and the vastitude of your knowledge in Botany. Your Manual of the Botany &c. was so useful to me that this winter I have determined and ascertained more than seven hundred species of phanerogamou, collected last year in the Ohio &c. what I could now have done on European plants with European Descriptions. The same correctness I have seen by Mr. Sullivant and I am afraid that European people shall never be able to do something valuable on American Botany after you and our able friend the Phycologist of Columbus. On this persuasion, I am determined never to send to Europe a single plant before it has been examined by you or by Mr. Sullivant or by myself when I shall be able to do it. It is no possible to study one plant when one has not seen it growing in its natural place; when one was not able to compare it with the many variety of forms to which every one is liable. According to that purpose, I shall have very often to send you some doubtful species & I hope you will no deny of looking at those plants for, as much as possible, I will send you but the interesting and rare ones. You have heard probably of the determination of Mr. Sullivant who don't appear to want me more at his home and for himself. Since the first of March I shall be free and do my best for myself. I was first a little uneasy for my future living, but I trust to the Almighty Providence who gives the food to every bird. If I do my best, the Lord will certainly help. Before parting, Mr. Sullivant proposed to me to go South and to explore the pinebarrens of Alabama and after the mountains of Georgia and Carolina. I have accepted this proposal with great pleasure for I could never abandon the botany and perhaps this journey shall prove to be an advantage to me. For

as I collected ~~some~~ the last year a great many plants in the Ohio Co. I think to increase the number of my specimens and to sale them in America or in Europe. I would like to publish first a good specimen collection of mosses and hepatics of which I have three to four hundred American species in numerous specimens. For I collected myself here three hundred and Mr. Sullivant who has bought the Oakes's collection has intent to give me a great many of specimens. This collection would be, I think, very usefull, for as monography, the mosses were but well published by Mr. Sullivant and his worth is not to be sold as you know. With your precious recommendation, that of Mr. Agassiz and Mr. Sullivant, I could perhaps sale some of those collections of which I would give before a good catalogue as prospectus. I would like to have your advice on this subject. Surely, I am willing to collect much in the South. Mr. Sullivant thought I ought to go there at the beginning of March and return home after a journey of five or six weeks. But this would be a lost journey. I like more to go south, to stay there some months and to examine the country carefully. Mr. S. can pay what he is willing to that, and I will support myself the expenses. Did you have some advice or recommendations to give me to that journey, I would be very happy to receive them from you before starting, viz the first of March.

I can tell you nothing of Mr. Sullivant. You know him, and as you have told me it, I have found him always very kind and benevolent for me. I think our relations ought not to be longer, I see well I can not be of great use for him, as far as I am my company is more troublesome than usefull and at end, it is always better if one is free and can work for himself. If my essay is profitable, I would like to go next perhaps to the salt lakes for one year or more, so I think, I could be more usefull for you and for American botany as I am now in the Ohio Co. Nevertheless, dear sir, I give you a note of several plants that I have collected here and which are not named in Sullivant's and Bigelow's catalogues. Did you find something interesting in this note and wish to have specimens of the plants I will be happy to send you every thing I have.

Excuse this letter, my uncorrect language and to agree these words of sincere friendship and devotedness.

Respectfully yours

Alfred Lesqueray

P.S. Had you some opportunity to write to Mr. Carey who was so benevolent with me, please to remember me to him. He promised to give me a series of his papers. I would be very grateful of such a gift which would be of great use to me. Please also to send him my best regards.

Sub of phanerogamous plants collected in Ohio Co. 1844. not named by S.B.
Asclepias tuberosa Willd. very scarce near Columbus in the woods.
Syringodendron bulbifera L. as North as Mt. Vernon.
Nelumbium luteum Willd. In the canal 5 miles south of Columbus. I have collected seeds. Did you want for your garden?

Viola canadensis L. Lancaster County.
Parnassia parviflora DC. Niagara fall.
Hypericum sphaerocarpon Michx. Banks of the river, on chalk rocks.
Acer pensylvanicum L. Rock Hill, Lancaster Co.
Fraxinus medium L. ? Ohio, dry woods, south of Columbus, (resembles the European form).
F. - stoloniferum Mill! L. L. L.
Ludwigia polycarpa Ell. In river, & Columbus. Marshes.
Proserpinaca palustris L. Near Lancaster.
Hedera longifolia Moench. Near Lancaster on rocks.
Kuhnia eupatorioides L. Crest of the river near Columbus on rocks.
Aster patens Nutt. Near Columbus.
A. - undulatus L. L. L.
A. - sagittifolius Willd. L. L.
A. - multiflorus Nutt. L. L.
A. - junceus L. L. L.
A. - Tradescant. L. Marshes L.
A. - miser. L. L. L.
Dryopteris monanthoides Forst. & Gr. Lancaster Co.
Solidago viridifolia L. Lancaster Co.
S. - latifolia L. Columbus, in the woods.
S. - speciosa Nutt. Ohio.
Erigeron philadelphicus Michx. is very common on dry hills Lancaster Co.
Cynthis virginica Ba. Beyond Columbus.
Hieracium scaberrimum Michx. Forgotten the locality.
Myrica latifolia Nutt. Lancaster & Columbus, in the woods.
Utricularia subulata L. Near Lancaster, very fine specimen. The
U. - intermedia Hayne. is very common on the cranberry marsh near Upper Sandusky.
Silene acaulis Nutt. Columbus, woods.
Festuca ovina L. & F. lineare Nutt. Near Lancaster.
Abies balsamea L. Lancaster Co.
Polygonum tenue Michx. L. L.
Zanichellia palustris L. L. L.
Polypodium heterophyllum, P. pauciflorum. P. polypodioides & C. Ohio

Maerobylin ophioglossoides Nutt. Woods Columbus.
Corallorhiza odontorhiza Nutt. — — — — —
Platanthera hookeri Lindl. *P. bracteata* Torr. *P. dilatata* and *P. glauca* North Ohio.
Oxgonia pendula Lindl. Woods near Columbus.
Chamaelirium luteum Gray Hills near Mt Vernon.
Juncus acuminatus Michx. Cranberry marshes Ohio.
Cyperus rotundus Torr. var. marshy woods near Columbus.
C. retrofractus Torr? Cranberry marshes near New Haven.
Eleocharis pygmaea Torr. Bows of the Scioto river Columbus.
Stygnospora gracilicaulis Torr. Marshes Ohio.
St. — glomerata Vahl. New York. (It goes not there).
Abopocurus aristulatus Michx.
Agrostis perennans Gray & A. scabra. Willd. Ohio.
Eleocharis pennsylvanica Gray. In the woods. Ohio.
Poa sylvestris & *Poa pungens*. Ohio.
Panicum glabrum Gray. Hills near Lancaster.
Camptocarpus rhizophyllus Lindl. Lancaster. Pa.
Asplenium pinnatifidum Nutt. — — — — —
A. — ebenum. Nutt. — — — — —
Batrachium linearis Swartz. Columbus. low woods.
Ophioglossum sagittatum — — — — —

See also: I name you these plants of memory having no time
 to look at my collection. There is probably some errors but a great
 many plants I don't name here. For the mosses I collected in
 Ohio Pa. & several fine species which were not found again in this
 country six new for America and one or two I think new ones.

Columbus. 18^e Janv. 1852.
Prof. Gray Cambridge.

Cher Monsieur!

Depuis que votre retour d'Europe m'a été annoncé par un journal, et c'est, j'ai voulu maintes fois prendre la liberté de vous écrire tant j'en ai toujours éprouvé le désir de n'être pas entièrement oublié de vous ou pour mieux dire, de réclamer la part de bienveillante amitié que vous avez bien voulu m'accorder. La crainte de vous faire perdre quelques minutes d'un temps précieux m'a retenu. En arrivant à Cambridge, vous m'avez dû trouver entassée une énorme quantité de matériaux intéressants. J'ai conséquemment beaucoup plus d'ouvrage même que vous n'en aviez promis. J'ai reçu, il y a quelques jours, votre brochure: Notices of Botanical Literature &c. J'en suis très satisfait et vous m'avez accordé cette nouvelle preuve d'intérêt qui me fait un plaisir extrême, je me crois presque autorisé à vous envoyer quelques paroles de remerciements & à vous raconter ce qui m'est arrivé depuis votre départ, botaniquement parlant. D'abord j'ai lu avec un vif intérêt les nouvelles botaniques que vous rapportez d'Europe. J'ai parlé de quelques-unes à quelques amis, j'ai pu vous suivre dans votre itinéraire même dans vos recherches. C'est ainsi que M^r Duby, Godet, Shuttleworth, m'ont parlé au beau coup d'enthousiasme du plaisir que leur a causé votre visite & que M^r Godet m'ait dit il y a quelques semaines, qu'il a reçu de vous un charmant envoi de plantes de Floride en échange de ses plantes Jurassiennes. Je voudrais bien à ce propos que vous pussiez vous souvenir

survey des herbes de Sensybarie. Elle me donne beaucoup
à faire, puis qu'il faut se remettre à une étude long temps négligée
à quel y a énormément de matériaux à recueillir. Le examine
pour faire connaître un peu la richesse botanique de l'Amérique
du Nord. Le mariage coïncide comme un commencement,
que comme un chemin d'activité que St. Providence m'a ouvert
dans ma nouvelle patrie. Que je ne sois pas à la hauteur de
l'entreprise, je le sais; mais il en aura peut être comme avec
les mousses. Le travail me fera obtenir peu à peu ce qui me
manque encore. - Le voc. a, je crois bien longuement
à son inutilement ennuyeux de mes petits travaux, cher Monsieur.
Je crois parler à un ami. Je suis presque sûr que vous
accueillerez ma lettre avec bienveillance.

J'ai reçu il y a quinze jours déjà la fin du Synopsis
de Muller, comprenant l'appendice et les tables. Cet ouvrage
me plaît extrêmement. Il est de petit nombre de ces livres
qu'on aime d'autant mieux qu'on les étudie davantage. Il
est fait avec une telle exactitude de St. Sullivan, j'ai
entièrement abandonné la manière de Schimper dont j'étais un
peu fatigué. Il ne base ses espèces sur des caractères pas trop légers.
Schimper lui ne fait plus rien. Tourment de mélancolie,
une malice incompréhensible dans sa position de nouveau
marier. Il inspire ses travaux passés, blasphème (curse) la
trilogie qui l'a conduit dans des embarras pécuniaires intolérables.
Il propose d'abandonner la botanique pour étudier de la
paleontologie. Ce serait un grand malheur. Mais comme
vous savez dit St. Sullivan, la botanique en Europe semble
frappée d'interdit. Ceux qui la poursuivraient avec le plus
de talent meurent de l'abandonnement. Notre ami Godet
voudrait ajouter à la flore du Jura la description de mousses
hépatiques, à une demande de la travaille pour lui. Il lâche.
St. Sullivan qu'il a vu hier m'annonce un paquet de plantes
et la part de M. Parry. Je me réjouis extrêmement de recevoir
permettez moi, cher Monsieur, de vous serrer la main de tout
mon cœur. Je vous offre l'assurance de mon affection éternelle. Le Docteur

Columbus 27^e Oct. 1852.

M. Moench

C'est en rentrant chez moi samedi dernier seulement, après un séjour de 5 mois sans le bassin houiller de Pennsylvania, Ohio & Virginie, que j'ai trouvé votre aimable petite lettre avec le note de *Desiderata* de notes excellentes muni M. Boissier. Comme votre lettre est sans date, je ne sais à quelle époque elle m'a été adressée; mais j'en suis très vivement regrette de l'avoir obtenue si tard; car un mois ou deux auparavant j'aurais pu récolter presque toutes les plantes indiquées dans la note. Comme il en est, vous savez mieux que personne que maintenant tout trace de végétation est à peu près effacée, il est extrêmement difficile de trouver les plantes, même avec la connaissance de localités. Ajoutez que je suis devenu malade au legs & qu'il est difficile de courir les bois avec la fièvre & la dysenterie. Malgré tout, j'ai réussi à collecter dans nos environs une vingtaine de bonnes espèces que je vous ai expédiées aujourd'hui par express. — *Cardamine rhomboidea* ? racine. — Je ne suis pas très sûr de l'espèce. — *Viola striata* rac. *Viola pubescens*, rac. *Silene concolor* rac. *Nabalus Fraseni* rac. & graine. *Nabalus racemosus*, rac. & gr. *Heuchera Americana* rac. *Parnassia Caroliniana* rac. & gr. *Anemone Pennsylvania* rac. *Helene Pennsylvania* rac. *Polygonatum canaliculatum* rac. & gr. J'ai préparé les graines de cette espèce comme je prépare l'ordinaire les graines avec mucilage, en les lavant à l'eau froide. Elles contiennent bien l'ordinaire. *Amilacina stellata* rac. & gr. *Ocularia lanceolata* rac. & gr. *Mulsaria perfoliata* ? ou *grandiflora* ? *Amilacina racemosa* rac. & gr.

Sopfieldia glutinosa var. *Spignadenus glaucus* var. *Allium*
cernuum var. *sg.* *Pteris atropurpurea* & *Asplenium*
angustifolium. J'ajoute encore de mon jardin *Sedum tele*
phoides. J'ai au jardin une quantité de ces espèces, mais
comme j'ai été absent tout l'été, je n'en retrouve plus la
place parce que je n'ai pu y planter les oquettes au printemps.
Et comme je l'espère, je puis faire encore quelques courses la
semaine prochaine, je vous enverrai une seconde caisse, mais
il n'y faut pas compter, le temps devient mauvais & les
promenades difficiles. L'année prochaine, je pourrai
sans doute procurer pour M. Boissier à peu près toutes les
plantes qu'il demande, excepté celles de New Jersey &
je continuerai à mettre à mon jardin tout ce que je trouve
d'intéressant, de sorte que si jamais vous avez besoin de
quelque chose, je pourrai vous le fournir. Et je serai
très heureux de le faire, je vous assure, cher Monsieur, &
je pourrai vous être agréable en quelques choses. Votre bien
villante est si grande pour moi que chaque jour ma dette
l'accroît envers vous. Aussi, je ne puis comprendre com-
ment vous me demandez de payer mes peines ou mes
courses qui n'ont aucune valeur. J'ai votre bonne amitié,
j'ai trouvé aussi chez moi avec votre lettre les Plantes de Wright
qui m'ont fait un plaisir extrême & que je n'ai pas
encore fini d'étudier. Je vous en remercie de tout mon
cœur. Et malgré tout vous persistez à penser que l'un ou
de l'autre soit de quelque valeur pour vous, je me recom-
manderai pour quelques graines de plantes rares quand
vous en aurez de reste. J'ai envie de cultiver dans mon

petit jardin le plus grand nombre possible de plantes américai-
nes & peu à peu, il deviendra intéressant, pour moi du moins.
J'ai vu notre excellent ami M. Sullivan ce matin, mais il
n'a rien à vous envoyer. Et de vous adresser des graines que
nous avons recueillies autour de Columbus & des fruits d'*Asculus*
glabra que j'ai pris au sud de l'Ohio. — Merci encore cher
Monsieur pour votre bienveillante invitation. Je dois passer
quelques jours à Boston cet hiver chez le Prof. Rogers en con-
sultes géologiques & je ne me réjouis guère de ce voyage, que
parce qu'il me procurera le plaisir de vous voir. En printemps
j'irai emporter à Columbus un paquet de plantes que je devrai
vous soumettre & que j'ai remis à M. Carey à New York.
Comme il y avait beaucoup de carex & de très beaux *Stexamen*.
Il trouve un nouveau Carex des environs de Columbus & pense
que mon *Senecio* de Vashville est en effet une nouvelle
espèce. J'ai encore quelques échantillons de ces plantes de Malabar
et vous en devriez être tout à fait sûr. Aussi, je vous
offrirai de grand-cœur les plantes de la contrée qui habite
maintenant & des parties de l'Amérique que j'ai visitées. J'ai
trouvés abondamment le *Lygodium palmatum* au dessus de
Mauch Chunk, Lehigh Co. Pa. Mais il ne croît pas dans
les rochers. Il pousse dans les bois & broussailles des marais &
grimpe aux branches. Il était trop jeune quand je l'ai récolté
& je n'ai pu en avoir le fruit. Je prends la liberté de
mettre de ma lettre une *Viola* de bois de Pennsylvanie. Je ne
sais ce quelle peut être si ce n'est une var. de *Lucilla*.
Elle se rapproche du *V. Calceolaria* du *V. Selkirkii* mais elle a les
pétales barbus. (Pétales bleus). Je ne sais non plus que faire

de cette Commelina qui n'est peut être qu'une rareté l'ardine
de P. Virginie. Elle croît & le sol humide au dessus de Gallipoli
Ohio, les tiges sont tout à fait prostrées, rampantes. Les radicaux
sont nuds; les feuilles diffèrent tout à fait - -

Je suis honteux de vous occuper si longtemps pour si peu
de chose, cher Monnier. Excusez moi en songeant au plaisir
que je procure à chacun au moment où vous & notre chère
science - De cœur la plus sincère considération,
votre ami dévoué
L. Desquereux

Les graines sont dans le papier cassé au dessus
de la cire. Faut il vous renvoyer la note de M^r
Bairner ou la garder pour récoller les espèces. Je pourrais
en prendre copie si vous aviez d'autres correspondants qui
pourraient recueillir les plantes.

Columbus. D. 14^e Joul 1853.

Cher Honneur.

Il faut que vous ayez bonne patience pour attendre
ans vous en plaignre, la réponse à votre aimable lettre
du 15 Juillet. Le renvoi de la lettre de notre ami Godet.
J'avais envie de trouver un bon moment pour vous
envoyer de cette lettre et quoque present il m'a été impos-
sible de le faire. Les affaires pressantes. - Une correspondance
de quelques uns m'ont pris tout mon temps.
Je vous avoue tout franchement que je ne suis pas content
d'aucun de la lettre de M. Godet. Il est de l'école de Her-
bach, de Gsch, de l'ami Hecker de Genève, de la nouvelle
école en un mot, qui ne nous permet plus de rien re-
tenir comme identiques à ce qui a été fait ou dit
jusqu'à present et retourne tout à fait le vieux proverbe,
qui n'y a rien de nouveau sous le soleil. Je sais qu'on
ne peut établir de limites à l'esprit de l'écriture
à l'homme que la manière d'entendre le progrès
mais qu'en qu'on affaire individuelle. Je comprend et j'
a des doutes à tout et me semble. Je me rappelle
les espers, d'Amérique, nos naturalistes d'Europe ne font
pas sans la part des circonstances locales. Un fait
qui frappe tous ceux qui étudient par comparaison la
faune d'Amérique celle d'Europe est, comme bien vous le
savez, cette prépondérance d'une seule espèce, qui tient
cependant toujours quelque chose d'incertain, de non
défini. Il n'y a pas dans le phanogame une seule

espèce qui présente absolument les mêmes caractères
que son homonyme d'Europe. Le même fait se présente
dans le moussin, c'est-à-dire une espèce de persécution.
Je retrouve également dans la flore fautive des deux
continents. Que conclure? Ou bien, quitte tout
appareil, faire deux flores. Ou bien les mêler en une seule
admettre une nomenclature absolue et américaine,
ce qui est impossible puisque le système ne peut se séparer
des mœurs; ou bien, quitte tout être un peu plus soignant
sur les caractères de peu d'importance et les mettre sous le
simple des influences locales. Tout ce que vous avez fait
pour votre flore, que j'approuve entièrement et qui l'honneur
très-délicat semblerait vous reprocher. Je reviens le voir
et seulement deux ans sur plus de lecture des travaux
imposés. Quand j'arrivais chez M. Muller, tout
plein d'enthousiasme pour mes déterminations, j'admettais
toutes les espèces comme nouvelles. Grâce à notre savant
ami biologiste de Columbus, ce fut bien du même
esprit. Mais, tout cela, serait je suppose, tomber main-
tenant dans l'écueil contraire. Mais il n'y a pas grand
mal à cela, scientifiquement parlant, le rôle sera le même
et tout. — Un autre reproche que je fais
à notre ami Godek, c'est de ne pas oser ou ne vouloir
se charger franchement de la responsabilité d'auteur.
Je ne vois pas pourquoi on comment il est utile
de décrire des espèces nouvelles par d'autres pour docteurs
savants que ces espèces ne sont que des variétés dans

l'opinion de l'auteur. Bien sûr, on multiplie les espèces,
on fait un gros livre, c'est vrai; mais c'est là tout le
qu'on peut dire de mieux en forme de la méthode. —
Je crois maintenant qu'en passant du général au particulier
on trouverait les observations de M. Godek en concordance avec
la méthode. Comme exemple, note *Uspidium aculeatum*
vous en citez un de M. Trautner, je ne sais pas si c'est
il vous renvoie pour cela à la flore p 883 ou il dit que
M. Trautner lui paraît jusqu'à présent un ordre d'être
conservé. — D'ici là, malgré l'autorité de M. Godek, c'est
à M. Godek sans doute de le voir que les fait être, note
ami non trouve aucun qui soit identique à ceux d'Europe.
Le rôle par contre que note les super-abondances qui s'opposent
certes de note les variations par tout autant de ^{caractères} ~~caractères~~
que les ceux se trouve que parfaitement le S. variations
d'Europe. M. Muller est maintenant p. cette espèce
dans l'ouvrage. Si cela avait le moindre intérêt, je
pourrais en faire des observations. Mais à quoi bon. Le rôle
suffisamment à propos de moussin, par les innovations constan-
ment répétées de note ami Schimper qui lui aussi
trouve du nouveau partout quand Muller décrit com-
me espèces, anciennement décrites, des formes qui sont
certainement distinctes. De là aussi on peut en dire
répéter. M. Muller, qui est inutile de le répéter
sur notre flore américaine, de l'opinion de l'auteur,
qui change plus d'une fois le fait en la même espèce
matérielle obtenue en grand nombre de ceux-ci.

Columbus. D. 10^e Juillet
1853.

Cher Monsieur.

Depuis fort longtemps je me suis abstenu de vous importuner de mes lettres. Comme tout votre temps est employé à des recherches scientifiques d'une grande importance, que votre bienveillance pour moi est toute gratuite, puisque dans mon humble opéra, je n'ai jamais rien fait & ne pourrai probablement jamais rien faire pour le mérite; la meilleure preuve d'affection & de respect que je puisse vous donner ce me semble c'est de vous épargner des visites & des causeries qui n'ont aucun intérêt direct pour vous. J'ai vu cependant avant hier chez M^r d'Allouant notre digne ami une lettre de vous, où vous parlez de mes différends avec M^r Rogers en termes qui m'ont fait trop de plaisir pour que je ne me hâte pas de vous en remercier. Certes, si j'eusse supposé avoir le moindre tort à l'égard de quelqu'un & surtout envers un homme pour lequel j'ai travaillé & qui a eu pour moi quelques bons offices, je m'empresse-rais de les réparer. Mais je crois au contraire avoir fait à M^r Rogers trop de concessions & c'est parce que je me suis arrêté à temps encore, que la rupture a été tout près de se faire. Vous ne pas vous ennuyer inutilement de détails qui ne peuvent vous intéresser, je vous en salue d'affaire d'une plus grande simplicité. D'un accord écrit & signé de M^r Rogers, M^r s'engage à me payer

\$600 pour un travail sur les plantes fossiles de Pennsylvanie
pour l'année 1852-53. à raison de \$100 payable chaque
deux mois du 1^{er} May 1852 au 1^{er} May 1853. Pour cette
somme il demande cinq mois de recherches dans le
basin houiller de Pennsylvanie suivant les localités où il
lui semblera bon de m'envoyer. 2 deux ou trois mois de
travail de cabinet pour mettre les matériaux en ordre.
publier les plantes fossiles &c: faire le rapport ^{le tout à mes frais} en un mot.
Je n'ai pas besoin de vous dire que Rogers ne connaissant
absolument rien aux travaux géologiques ne peut se faire
une idée de temps qui demande l'étude & la publication
des plantes fossiles. Mais comme j'acceptais la proposition
pour la science tout seulement & nullement pour l'argent,
je ne me suis point inquiété de temps que le travail demanderait.
J'ai maintenant donné à la Survey de Pennsylvanie cinq
mois & demi de recherches sur place (field work) &
six à sept mois de travail de cabinet & reçu tout
seulement \$300 tout juste l'argent dépensé dans
mes courses d'explorations. De sorte que pour tout le
travail fait & à faire car je n'ai pas fini, je n'ai pas
encore reçu un centime de rétribution. Il est vrai que
je n'ai rien encore écrit à M. Rogers. Et c'est pour cette
seule considération que je prends patience sur toutes les
petites chicanes qu'il me fait. Il a encore été ici le
dernier dimanche, il trouve les débris magnifiques &
m'a promis le paiement de ce qu'il me doit quand

j'aurai fini l'ouvrage, en octobre ou novembre. L'un autre
côté je lui avais écrit qu'il n'aurait ni échantillons ni rapport
avant qu'il ne m'ait payé ou que je n'aie la certitude de
recevoir mon argent. J'étais moi-même à Boston & l'épouse
où je serai prêt à livrer le travail. J'aurai soin de le faire
examiner par les meilleures autorités. Vous serez le premier
comme de juste. Toutes ces tracasseries m'ont complètement
dégouté de chercher à me faire une existence (à living)
par la science. J'ai commencé avec mes fils ici un petit
commerce d'horlogerie & j'en ferai mon gagne pain. J'ai
en cependant un immense avantage & de grandes jouissances
à l'étude de cette belle flore fossile américaine & il y a
tant de choses curieuses & nouvelles & si les poursuivre avec
amour. J'espère que l'ouvrage, qui aura une vingtaine de
planches, aura votre approbation. Je crains cependant que
Rogers ne le publie pas avec les notes nécessaires & même
comme je connais l'homme, je crains qu'il ne le publie jamais.
Je ne finisse par se l'approprier. Il y a bien des gens qui
aiment mieux éteindre une chandelle que d'en laisser la
lumière aux autres. En tout cas, j'aurai toujours eu le
plaisir de travailler & Rogers lui-même ne saurait me priver
de non plus que les connaissances acquises par cette étude.

Depuis tantôt deux ans, j'ai fait rendre ou
donné à des amis plus de vingt exemplaires de votre
flore des Etats Unis (Gray's manual). Quand même
je n'aurais pas l'honneur de vous connaître, Mr. Morrison
je ne pourrais m'empêcher de reconnaître que c'est le meilleur

ouvrage de botanique pour l'étude de plantes d'Amérique
de Nord. Je n'est pas je vous assure l'affection sincère d'élèves
qui j'espère pour vous, mais simple justice qui me port
à dire à tous les étudiants qu'il ne les remplace tous les autres.
J'ai vu dans une lettre à M. Schimper que votre seconde édition
est retardée par la route trop lente de la première. Il faut
que vous ayez un très mauvais libraire. Et tout cela, il y a
un mois qu'il n'y avait pas à Cincinnati, dans toutes les
librairies un seul exemplaire de votre flore. J'en avais promis
un à un ami, mais j'ai cherché en vain. Il a fallu en
faire venir un de Columbus. Celui que j'ai obtenu était
le seul exemplaire qui restait ici. Maintenant on ne pourra
en trouver un ni à Cincinnati ni à Columbus.

Vous avez appris le mort de mon pauvre excellent ami
Scharen, mort de désespoir d'avoir publié un livre auquel il a
employé tout sa vie. Il n'est troué à sa apparition et
arrière de la science pour qu'il avait repoussé l'aide de mi
croscopie. Excellent ami m'avait légué toutes ses collections
d'herbiers de distribution. Car mon absence d'Europe, M. Gutsmuth
a été chargé de ce triste devoir.

Vous avez reçu aussi le second volume de la flore de notre
ami Godet. Je n'ai pas de critique à en faire, cependant
je n'aime pas cette méthode de Reichenbach. Les subdivisions
sont à l'infinitif de genres et espèces. Schimper fait de
même pour les hypnaces des mousses, et opposé à von Mueller
qui avait tout réuni. La vérité est relative, le vent du fait
d'avoir foi en ses propres œuvres ou botaniquement parlant
en ses propres espèces. - Comme je ne trouve ni à rendre
ni à donner mes collections de plantes phanérogames d'Europe
que les vers mangent ici, je vais les renvoyer en Europe
pour en faire présent à quelque musée, il y en a environ
7000, je garde la flore d'Amérique. Est-elle. Il y avait
dans ces plantes des espèces qui pussent vous intéresser, je serai
heureux de vous les offrir. - De ces votre devoir. Le docteur Schimper

Columbus 0 May 21 1855

Cher Monsieur.

Notre ami Guyot, je l'espère vous aura
de ma part annoncé mon prochain voyage en Suisse.
Il est tout à fait inutile de vous dire que j'
serais heureux si je pourrais vous être utile ou
agréable en quelque chose. Je verrai sans nul doute
notre ami Godet qui m'écrit toujours souvent
probablement aussi Mons^r Boissier à qui j'
porte des bulbes du *Silla esculenta* & des plantes
de *Sedum pulchellum*, *ternatum* &c. J'ai aujour-
d'hui des plantes du *Lypripedium spectabile* qui
poussent très bien au printemps & ont abondamment
flori cet automne. Je serais bien aise d'en prendre
avec moi; mais je crois qu'à cette saison & proli-
ment au moment où les plantes commencent
à sortir de terre, le transport serait impossible.
J'avais l'automne dernier apporté de Nashville &
mis au jardin la *Heuchera villosa*? de Nashville
une bien jolie plante qui fleurit en septembre.
Elle a péri cet hiver, du moins je ne puis la
retrouver. — J'ai passé au printemps quelques
jours à Nashville pour affaires. J'ai eu l'occasion
d'y recueillir ce *Vesicaria*? prononcé nouveau par
M. Carey & autres botanistes. J'ai préparé pour
vous les meilleurs échantillons c'est à dire
ceux dont les fruits étaient les plus murs,
afin qu'il vous fût possible d'étudier cette plante
qui, bien que je n'en trouve nulle part la des-
cription, a l'apparence d'une plante connue.
J'ai ajouté au tout petit paquet deux ou
trois autres plantes de Nashville, entr'autres
le *Allium triflorum* & le *Spermatocoea glabra*,
trouvés à Cincinnati d'où vous me l'avez

comme plante nouvelle. Il y a quelques
mauvais échantillons d'une petite rubiacée,
cueillie sur les bords de la rivière à Washville.
Elle rampe sur le limon en septembre. - Comme
le tout petit paquet ne vaudrait pas le port,
(je le donnerai aujourd'hui à M. Sullivan &
s'il n'a pas d'occasion d'rien à envoyer à
Cambridge, je le porterai la semaine prochaine
à Newyork. Mais là, à qui le donner ?
Je dois quitter Columbus le 28^e ou 29^e &
prendre passage de Newyork sur le Arago le 2^e juil.
Si donc, cher Monsieur vous avez quelque chose
à me communiquer, quelque commission à
me donner, avec la bonté d'adresser vos lettres
& paquet à mon nom (care of Henry & Droy
92 Fulton St. Newyork). Je ne m'arrêterai en
Angleterre qu'en revenant au mois de septembre.

Vous apprendrez sans doute avec plaisir
que j'emporte avec moi pour Schimper la
un exemplaire de nos mousses d'Amérique.
Nous sommes donc à peu près prêts mais
ce n'est pas sans un travail vraiment
considérable. Nous avons passé tout l'hiver
à revoir les Hypnacees pour les mettre
si possible au niveau du magnifique travail
de M. Schimper. Vous pourrons publier
le manuscrit, étiquettes &c. : tout après mon
retour d'Europe & alors mon premier soin
sera de vous adresser un exemplaire de
ce travail qui, bien que de minime
importance ne sera pas sans valeur pour
la botanique Américaine. M. Sullivan
va très bien & il a peu travaillé beaucoup
cet hiver. Je n'ai pas besoin de vous
dire que je lui fais visite aussi souvent

que possible.

Veuillez je vous prie, cher Monsieur,
me rappeler au bienveillant souvenir de
madame Gray & agréer l'expression de
mon affection sincère & respectueuse.

Votre tout dévoué

Leo Lesqueren

Columbus 21th May 1855

Dear Sir.

I wrote you this morning and this evening I have the pleasure to receive a letter from you. I am acquainted with M. Springiell only by what friend Godet wrote to me about him. He called on me last week and I was truly pleased with his manner and deportment. He looks like a good, healthy and temperent man. This, M. Godet writes about him. — "Voilà Denis Springiell qui le premier avait fait son apprentissage au jardin botanique de Neuchâtel, & qui s'était établi à Berne qui va partir lundi & voir s'il n'y a rien à faire au delà des mers. C'est un excellent jardinier qui s'embarque avec quelques fonds & que je vous recommande. Il est très laborieux & est prêt à tout faire pour se tenir d'affaires." I should think from what M. Godet writes, from what I have seen myself and from the certificates that he has received, that this M. S. would suit you exactly. Nevertheless, as I am to be in Europe in about 4 weeks, I could make such inquiries about him and write you immediately and you could

then act accordingly. I have news from him. He has found immediately something to do at Philadelphia where he was well recommended.

I have seen Mr. Sullivant this morning. He has received your package and is very happy about it. But I am entirely ignorant of a letter from Godet. I never received it through Mr. Sullivant. You sent me one, containing a long critical analysis of your *Am. Species*. but I ought to have returned it to you and I think I did it. I will go to night to Mr. Sullivant and send it if I receive it.

Mr. Sullivant has nothing to send to Cambridge so I rather choose to mail for you these few plants of which I spoke this morning.

I am much obliged to you for the paper of Mr. Al. Braun. I would that I could send you also something. If I had had time I would have not only a note about some plants especially some localities for the second edition of your *Flora*. But you are well aware that I am rather a poor adviser. In any case I am

certain that your second edition like the first will be a very good very useful book and will sell perfectly well. The mones want a great deal of corrections and especially of additions. I rather think that Mr. Sullivant had better to publish a good synopsis of American mones and Hepaticae. But he is far from being ready for this work. I hope to find at New York a letter from you and also your desiderata for Europe.

Very respectfully your friend
De Lesquerens

Mr. Sullivant sends the very pleasant letter of Godet. He had forgotten it. All right now. I have read it but our friend Godet is much mistaken if he thinks that I have forgotten him. I shall take care of giving full proof of my friendship. I would gladly you sent me to New York an answer for him and some plants also if you have any for him.

New York 1^{er} Juin 1855

Cher Monsieur, Excellent ami.

J'ai reçu à Columbus & ici toutes vos bonnes lettres qui m'ont causé un vif plaisir. J'ai aussi reçu le paquet de notre ami Godek. Il lui fera tout de plaisir que je suis tout heureux d'en être le porteur. Je ferai comme vous dites. Je mettrai aux messageries au Havre. Cela coûte très peu. Je remets moi-même aux messageries de mes propres effets tout ce qui n'est pas absolument nécessaire au voyage pour éviter les frais de surcharge. Le seul reproche que j'aie à vous faire, cher monsieur, c'est l'envoi de ces dollars que je considère qu'on ne doit pas en se mettant à votre compte pour l'avenir de penser.

Il est bien certain que le *Vericaria* de Nashville est un *Alyssum*. La capsule n'est renflée que pendant les deux premiers jours après le chute de pétales. De là elle est continuellement aplatie. A seulement un peu renflée à parfaite maturité. Comme j'en ai avec moi des échantillons, je la comparerai à Genève. Vous verrez le résultat. Surtout, en bien souvenir une foule de plantes que je croyais intéressantes à vous envoyer. La crainte de vous en nuire de choses inutiles m'a toujours retenu. Dès maintenant, je serai plus actif & vous procurerai plus de choses ou de plantes que je n'ai pu le faire jusqu'à présent.

Si Dieu me prête vie, toute fois. Quand
on s'embarque pour l'Europe, c'est par le
moment de faire des projets pour le
retour. J'ai reçu ici une bonne lettre
de Mr Sullivan à propos de quelques
mousses à comparer. Plusieurs bonnes lettres
aussi des excellents amis Agassiz & Gayol.
De sorte que je suis tout heureux de
voir un si bon nombre d'amis me tendre
la main au départ.

Ce serait un grand bonheur pour moi
de faire la connaissance de Mr Hooker.
Pareu. Peut être obtiendrais je quel
ques mousses pour Mr Sullivan. Si
vous daigniez m'envoyer une lettre pour
lui j'en serais extrêmement reconnaissant.
Mais ayez la bonté d'insister sur
l'insouciance de ma surdité afin que
Mr Hooker ne me revoie pas s'il ne
peut perdre son temps inutilement
pour moi. Vous pourriez remettre
votre lettre au Prof: Rogers qui part
de Boston le 19 Oct. & adresser votre
lettre Leo Lesquereux, Steurier
Canton de Neuchâtel, Suisse.

Ayez la bonté je vous prie de présen-
ter mes hommages respectueux à Mada-
me & agréer l'assurance de mon
amitié sincère & respectueux.

Leo Lesquereux

Vous m'obligeriez de dire dans l'occasion
à Monsieur Agassiz que j'ai reçu ses bonnes
lettres & sa caissette & que le tout arri-
vera à sa destination.

Columbus (I). 17th Dec 1855

Cher Monsieur

En arrivant ici d'Europe, j'ai eu l'honneur de vous écrire en vous envoyant le portrait de notre ami Godet, lequel vous auez bien reçu j'espère. M^r Sullivant m'ayant dit que vous étiez parti pour l'Europe & que vous ne reviendriez pas avant la fin de l'année; qu'il aurait d'ailleurs bientôt quelque chose à vous envoyer, j'ai cru qu'il était inutile de vous envoyer le paquet de M^r Godet. J'aurais à faire un voyage dans le Sud & ne suis revenu à Columbus qu'aujourd'hui. Voilà pourquoi le paquet a été retardé & pourqu'il auz je vous prie d'accepter mes excuses pour ce retard. Vous trouverez dans le paquet quelques plantes intéressantes de Louisville & de l'Indiana & aussi quelques brochures de l'Europe.

Si j'en avais le temps j'aurais beaucoup à vous dire de nos amis d'Europe. J'ai reçu de M^r de Randolle sa Géographie des plantes, mais n'ai pas encore eu le temps de l'étudier. L'examen de quelques espèces de mousses qu'il faut recommencer pour terminer nos Musci endocati. Liner. & aussi pour le travail, (le magnifique travail) de M^r Sullivant pour votre nouvelle édition prendra tout notre temps cet hiver. M^r Sullivant

me charge de vous dire qu'il vous écrira demain
ou après demain.

Permettez moi de présenter mes respects à
Madame Gray. Je vous offre l'expression
sincère de mon affectueux dévouement.

Leo Lesquereux

Columbus O. Janvier 12^e 1856.

Cher Monsieur

Voilà dans une de vos lettres à M^r Sullivant
"I think I told him that I had put the *Venicaria*?
"Lesquerensii" in the manual." Comme c'est la première
nouvelle, je m'empresse de vous en remercier de tout
mon cœur. Mon nom, quand il a été employé a
naturellement été latinisé en *Lescureus*. Ainsi
s'imprime à un genre *Lescurea* plusieurs espèces *Lescurei*
et M^r Sullivant a un *Bryum Lescurianum*.

Ayez la bonté de modifier le nom pour qu'il s'accorde
avec celui des autres auteurs. Je suis bien aise que
le paquet vous soit parvenu. Lui regrette beaucoup qu'il
vous ait été si longtemps retardé. Vous ai j. j. jamais
envoyé quelques notes sur les localités de votre collection.
Probablement ce serait trop tard maintenant.
Les moules de M^r Sullivant pour le manuel seront
magnifiques. Je suis pour mon compte bien heureux que
votre seconde édition se publie, autant pour obtenir le
ouvrage que pour en finir une fois avec nos *Musci*
exsecrati qui seront cités dans le texte.

Nous travaillons avec ardeur à la révision de toutes
les espèces douteuses. J'ai rien de recevoir encore les espèces
de M^r Lamas que vous avez envoyées. N'ayez de
nouveau et de bon genre *Aburhum* et une petite *Omalia*.
Mais tout cela a été déjà examiné en 1850. Malheureu-
sement le paquet est resté aux mains de M^r Sullivant
qui n'a pas le temps de s'occuper de tous ces détails.

J'espère que le bout de lettre vous trouvera vous
(Madame) en bonne santé ; que vous aurez joliment
commencé la nouvelle année. Que Dieu vous accorde
de la continuer de même.

Votre tout dévoué Sonner ami
Alo Lesquereux

Le roi qu'un de mes amis Allemand a
fait un genre d'animaux mi-croscopiques que je lui
avais fourni, sous le nom de Lesquereuxia. Mais
cela ne sonne pas bien. Puis qu'on emploie le nom
latin, autant vaut le prendre bon latin.

Columbus. 11. 16.^e Octbre 1856.

Cher Monsieur.

Je viens de recevoir par notre ami Monsieur
Cullivant un exemplaire de votre seconde édition de
Botany of the northern United States, et bien que j'en
aie à peine le temps de le parcourir, je m'empresse de
vous remercier et pour moi-même & pour la science de
ce beau livre que j'étais bien heureux de servir à votre
généreuse amitié. Il trouve un grand nombre de change-
ments qui me paraissent importants & d'une grande
utilité surtout pour les étudiants. Ce livre encouragera
& facilitera l'étude de la Botanique; car certes, autant
du moins que j'en puis juger, je ne connais aucun
travail élémentaire qui unisse autant de profondeur &
autant de clarté. Je serais bien heureux, je vous assure
cher Monsieur, si je pourais mériter votre bienveillance
par quelques communications intéressantes. Jusqu'à
présent, force que j'ai eue par ma pauvreté ou si vous
voulez par les difficultés de ma position, je n'ai pu
profiter de mes excursions botaniques de manière à
faire de bonnes collections de plantes phanérogames.
Mais si Dieu me prête vie & me laisse la santé, je
tâcherai à travailler avec plus de zèle & pour des
résultats plus importants. J'ai recollé cette année
au Minnesota & à Wastville quelques plantes que
je serais bien aise de vous soumettre. Je prendrai

la liberté de vous les adresser par express au trois
brasons des Jungi de Monsieur Parnet qui vous me-
rendrez grand service d'adresser au Pasteur Duby à Genève
avec les exemplaires des Musci & Hepatica etc. que Monsieur
Sullivant envoie à Genève. M. Duby pourra payer le
port de tout le paquet & se chargera volontiers de distribuer
aux exemplaires adressés en suite.

Vous aurez peut-être des nouvelles de notre ami Godet
que ces opinions royalistes viennent encore de mettre en
danger dans l'échauffourée, pour ne pas dire Révolution
Prussienne à Neuchâtel. Au moment où les gens des
montagnes ont repris le château de la ville, notre ami, bien
qu'il ne se fût mêlé de rien & craint d'être arrêté ou
même à craindre sa vie en danger s'il s'est réfugié chez son
voisin le Prof. Desor qui appartient au parti Républicain
où il a pu rester tranquille jusqu'à ce que l'échauffourée
fût passée. Ce n'est pas lui qui méritait, mais un ami
commun. Certes, ce n'est pas de nature à lui faire
aimer davantage Neuchâtel et je suis certain que s'il le
pourrait sans de trop grands sacrifices, il reviendrait volontiers
en Amérique avec sa famille. Mais l'Amérique offre-
t-elle maintenant plus de chances de tranquillité que
l'Europe? Il n'y a bientôt plus un coin au monde où l'on
soit sûr de trouver le calme que demandent les poursuites
de la science.

Ne pensez-vous pas que le magnifique travail de
Monsieur Sullivant sera admiré partout. Il le mérite

certes bien car depuis longtemps il a travaillé sans relâche
& n'a reculé devant aucun sacrifice pour l'avancement de notre
chère Bryologie. Les étiquettes des Musci Amer. insic.
vous seront envoyées cette semaine & je prendrai la liberté
de vous offrir le premier & le plus beau des exemplaires.
Vous verrez que le nombre des espèces dépasse quatre cents.

Ayez l'obligeance, cher Monsieur, de présenter mes
respectueux compliments à Madame Gray & d'agréer
pour vous même l'assurance de mon affectueux
dévouement.

Léon Lesquereux

Cumbe, N. Oct^h 21th 1856

Dear Sir

thought to find time and look again over these
specimens. Please excuse if I give you the trouble of determining
many of them: I have plenty of more common species
of the prairie. Do you want a list of them? I had
a bad time in W. Minnesota, being on foot and with
unlearned companions who did not want to wait for
my purpose. Moreover my largest & best bundle of
plants was carried away by a boat while I was
gone further west and I could not reach it anywhere.
If there is some species of which you want specimens, I
can send you at any time. I have corresponding numbers
with those of the labels.

Mr. Silliman sends to day the labels of Musci:
cane: etc: and also some labels for sending to Europe
his musci. Will you please forward to Pasture
Duby naturalis & Genera these three copies of
the fungi of Mr. Perout with the copy of the
Musc. & Hepatica that Mr. Silliman directs
to him.

I have worked hard this morning for the labels
and am now in a hurry to be at time for the
express. I wrote you a few days ago

Very respectfully yours.

A. S. Silliman.

Prof. Asa Gray Cambridge.

My young man forgot to put ^{the letter} in the package forwarded to you ^{by}
day per express (paid).

Columbus O. 1^{re} Fevri. 1857.

Cher Monsieur.

Le retard apporté à l'impression des étiquettes m'a permis de préparer au fur et à mesure qu'elles arrivaient 25 exemplaires de nos Musci exsiccati. De sorte qu'au moment où je recevrai les courantures des tables, je serai prêt à expédier les exemplaires qui me sont demandés. Je voudrais donc savoir ~~quel~~ combien d'exemplaires vous désirez à part le vôtre si que ils sont destinés. D'après votre lettre, vous en attendez trois; un p. M. Bormea, un p. C. Mues Delenest & un p. M. Lowell. Tout le reste vous en envoyez ou réservez d'autres. Je doute fort qu'il me soit possible de préparer cette année un plus grand nombre d'exemplaires, que les 25 qui sont prêts; le travail étant réellement trop rude & trop long puis qu'il faut que je fasse tout moi-même. Et en outre je n'ai pas de temps disponible.

M. Goe de Montreal a commis à payer un exemplaire. Puis-je vous l'adresser avec les vôtres. De même j'en envoie un à M. Barneul qui m'a fourni considérablement de chantillons.

Il me dit de vous l'envoyer pour le
lui faire parvenir par mes soins Boston.
Dois-je le faire? Serez-vous en prière,
prenez autant que possible l'envoi des
couvertures & des tables.

M. Sullivan est très content de
travailler. Ce sera une magnifique collection
mais si nous voulions compter le frais
que tous les deux nous avons fait pour
ce travail & le temps employé, chaque
exemplaire nous reviendrait au moins
à \$50. Vous pensez donc bien que
ce n'est pas une spéculation pour
le bénéfice, mais une affaire d'argent.

En vous priant de présenter mes
respectueux hommages à Madame Gray,
je vous envoie mes plus affectueuses
salutations. Les Desquereux.

Je n'ai pas de nouvelle de Godet
depuis six mois. Peut-être irai-je en
Turquie et etc.

Après vous avoir écrit le tout de
cette lettre j'ai fait visite à M. Sullivan qui
m'a fait lire la bonne lettre de M. Hooker.
Elle lui a fait un extrême plaisir & j'
le lui remercie sincèrement. Il s'est donné beaucoup
de peine pour obtenir quelques compensations.

J'écrirai à M. Bonnier & proposer de son
exemplaire qu'il pourra payer en suite.
Chacun des exemplaires portera un numéro
que je garderai avec le nom du propriétaire
pour que je puisse faire des rectifications ou
fournir de meilleurs échantillons s'il y a lieu.
S'il arrive que vous me direz un jour que
je n'entends plus parler depuis longtemps.

Le prix à \$25 - Vous nous rendrez grand service à Monsieur Sullivant & à moi, d'examiner la collection pour en dire votre avis. - J'ai emballé chaque collection avec assez de soin pour que vous puissiez sans crainte les remettre à l'express. Vous pourrez le voir par la route. Mais j'ai pu trouver de caisse pour le tout.

Le prix pour l'Europe est de f. 105 qui représentent bien \$20 avec la différence du change. - M. Sullivant ne se souvient d'aucun autre nom que ceux à qui les collections sont adressées. S'il y a d'autres demandes, vous m'en blâgerez de me les adresser le plus tôt possible puisque je préparerai de nouvelles collections s'il en manque.

Laissez moi encore vous remercier de votre bonne lettre, offrir à Madame Gray mes respectueux hommages & vous, cher Monsieur l'assurame de mon affectueux dévouement.

Léo Lesguereux

Amé Columbus 0. 17. Février 1857.

Cher Monsieur!

Je remets aujourd'hui à l'Américain express un gros ballot à votre adresse, contenant huit exemplaires de Musci-ensicati. A part celui de Monsieur Sorrell dont j'ignore l'adresse & qui porte le n. 14, tous sont marqués des noms des propriétaires. M. D. A. Fox de Montreal a payé le prix de l'exemplaire qui lui est destiné. Monsieur Boissier du Musée Delcourt peuvent payer en Suisse en adressant un mandat de f. 105 à Messieurs Sandoz, Berthoud banquiers à Neuchâtel. Si le Musée Delcourt préfère payer à Paris, il peut remettre cette valeur de f. 105 à Monsieur Georges Berthoud artiste Rue de Berlin n. 29. Quant aux propriétaires Américains, Messieurs Sorrell, Eaton & Olney, le meilleur mode de paiement parait qu'il est le

plus sur est d'envoyer à mon adresse
à Columbus un chèque de \$20 sur New York.
Je suis extrêmement peiné de vous donner
de nouveau embarras pour ces collections
quand vous avez déjà tant à faire. Mais
c'est vous mêmes qui me dites de vous
envoyer le tout. Quant à votre exem-
-plaire, n° 7, je suis trop heureux de
pouvoir vous offrir cette trop faible pen-
-se de ma reconnaissance & de mon af-
-fection pour retarder ce bonheur à l'année
prochaine ou peut-être je ne serai plus
de ce monde. Il me reste sept exemplai-
res à rendre & j'en aurai assez pour
l'année car je n'ai pas autant de confian-
-ce que vous à la vente facile.

Cette collection est cependant la
meilleure qui ait été publiée jusqu'à
présent pour les mousses. Si vous
voulez bien en faire rapport au jour-
-nal de M. Silliman, ayez la bonté
de faire remarquer que plus de la

moitié des espèces ont en capsule des
échantillons supplémentaires destinés à
l'étude comparative & ainsi d'une grande
utilité; que ces échantillons représentent
souvent des variétés remarquables de l'espèce.
Squills sont d'ordinaire plus parfaits
que les échantillons collés. Il y a cer-
-tainement quelques échantillons maigres;
ceux de Californie surtout récoltés par
M. Bigelow. Mais ils sont si intéres-
-sants au point de vue Géographique
que tous les Muscologistes nous sauront
gré de ne pas les avoir laissés de côté.
D'ailleurs ce sont des additions gratuites.
Puisque la Collection telle qu'elle était
préparée il y a un an devrait se compo-
ser de 350 espèces & variétés & coûter
\$30. L'œuvre maintenant nous l'avons
élevée à 416 espèces & bonnes variétés
& baissé le prix à \$20. Cependant
après les 25 premiers échantillons
exemplaires vendus, je pense élever

Columbus O. 21. Feby 1857.

Cher Monsieur.

M. Sullivan me remet votre billet du 9 P.^h. Vous aurez reçu une caisse contenant 8 exemplaires de nos collections. Je vous en adresse aujourd'hui 2 par express une p.^r M. Joseph Blake & l'autre pour W. B. Ward Esq.^r. Il y a encore dans votre avant dernière lettre trois noms que je ne puis pas bien lire; Hunt. Sprague of Boston & Bladbourne ?? Faut-il vous envoyer des exemplaires pour ces messieurs. S'il y a plus de 25 exemplaires demandés maintenant, je pourrai me faire aider & en préparer de suite un plus grand nombre.

J'avais emballé les 8 exemplaires en carton. Mais l'express a demandé une caisse pour plus de sécurité. M. Sartwell voudrait un exemplaire en échange de sa collection de 158 espèces de Carex. Comme mes plantes phanerogames sont mangées des vers au galetois faute de temps pour les examiner, je suis forcé de refuser d'en augmenter le nombre maintenant. Mais j'écris à Godet qui peut être trouvera la vente de ces Carex en Europe.

Avant un mois je recommencerai mes courses vagabondes je pense passer trois mois de l'été le long du bas Ohio pour l'étude des terrains houilliers & des lignites quaternaires du Kentucky. M. Dale Owen me fait de bonnes & avantageuses propositions. Ce sera une bonne occasion de voir les plantes de cette contrée.

Votre tout dévoué

Léo Lesquereux

Dec 21. 5. 1. 1.

22. July.

Dear Sir. I open my letter again. Since you send one copy of our mosses to England and may perhaps send more hereafter, I would ask you to order for me some books of which I am in great want.

1st Lindley (Y) & Hutton W. The fossil flora of Great Britain. London 1831.

2^d. Witham (H. T. M.) The internal structure of fossil vegetables found in the carboniferous & coalitic deposits of Great Britain. Wrimbourn 1833. 4th.

~~For~~ These two works, I would like to have this year; but I do not want them before the fall. If I may have them sooner, it is well. I will pay you immediately either cash or by ~~specie~~ copies of our mosses as you like.

Then I would like to know what cost
E. Mammal A collection of geological facts and practical observations (1834

Morris. A catalogue of British fossils.

and if M. Benbury has published something about fossil plants and if I could procure it somewhere.

Please excuse this trouble. I have nobody but you to whom I can address myself for this.

Prof. Rogers will do nothing for me some I decline to further work for his paleontology.

Ever yours

S. Lesquereux

Columbus Feb'y 26th 1857.

Prof. Asa Gray Cambridge

Dear Sir. Following your letter
to Mr. Suberant I have sent you
to day through American Express a
copy No 21 for Mr. & Candolle.
I am working hard to have 25
more copies ready. As I have put
my wife and daughter to the business
I hope to be ready in two weeks.

I have still four copies left.

I sent you No 7 for yourself

- No 10 for D. & Allan Fox Esq. Montreal
11 Daniel P. Eaton " New Haven
12. S. J. Olney " Providence
13 H. W. Harenet Aiken
14 Lowell Esq. Boston
15 Ed Boissier Geneva
16. B. Delenat Paris.
19. Joseph Blake Cumberland Co. N. H.
20 N. B. Ward Esq. England?
21. De Candolle Geneva.

The copy for Mr. Harenet is a present
to him. and D. & Allan Fox has
paid for his copy.

I am anxious to know what you
think about this collection.

Most truly & respectfully
your friend.

A. Sanguera

of Providence have acknowledged the
acceptance of our musci with many fine
compliments and commendations of the
worth. Judge & Lane has N. 22. the
University of Toronto N. 23 and M. Hooten
N. 24.

Most respectfully your friend

A. Sengueren

Columbus O. 7. Mars 1857

Mon Monsieur,

J'ai été si fort occupé toute cette semaine
à la préparation de 25 nouveaux exemplaires
de nos musci que je n'ai pu répondre à
votre aimable & bienveillante lettre du
24 Feby. - Laissez moi d'abord vous
remercier encore de tout mon cœur de la
peine que vous vous donnez pour faire
valoir & pour placer des exemplaires de
nos musci. C'est un véritable service d'amitié
que vous me rendez. Et la fin de la
semaine prochaine à 14 h. j'en aurai encore
25 exemplaires de prêts & comme je pense
alors quitter Columbus pour plusieurs mois,
vous m'obligeriez de me dire s'il faut en
core m'envoyer avant mon départ. En
tout cas, les exemplaires seront tous em-
ballés & pourront être expédiés sur l'ordre
de M. Sullivan. - Vous aurez reçu
les exemplaires p. Messrs. Blake, Ward
& de Candolle. - Messrs. D. C. Eaton &
A. Lowell m'ont envoyé le paiement de
leurs exemplaires & M. Sullivan a payé
celui de M. Engelmann qui est ici à la
disposition du propriétaire. M. Sullivan
a envoyé un exemplaire à M. Hooten.
Faut-il en adresser un à Prof.
Porter. Je ne sais pas où il habite

Vous m'obligeriez beaucoup de me
donner son adresse. Il ne me reste plus
qu'un expl. des 25 premiers que j'avais
préparés.

Si vous avez eu le temps d'examiner
les musci, vous me rendriez service de
me dire ce que vous en pensez; non
pas en compliment, mais en toute fran-
chise pour que je puisse faire les correc-
tions nécessaires. Je voudrais avoir des
doublets en capsules pour toutes les espèces,
mais ce n'est pas possible. A mesure que
je pourrai me procurer des échantillons, je
les distribuerai.

J'ai fort peu de temps pour écrire aujour-
d'hui. Vous prie d'agréer encore mes sincères
remerciements & l'assurance de mon
affectueux dévouement.

Léo Lesquereux

Just as I had wrote the above
I receive your very kind letter of the 3rd.
with a check for \$20 from M. Olney.
I can not express my gratitude for your
kindness. - About the books on fossil
plants, I mentioned them only because I
had the occasion of obtaining them. I have
had Lindley's in my possession more
than two years and am well acquainted

with his species and if I can have with me
from your library, sometimes when it is
wanted, I am very glad to spare the money
that it would cost. - This economizing
of yours for my sake is a new and
free service rendered to me. Prof D. De-
Bow has offered me an appointment for
the geological survey of the coal basin of
Kentucky and I think of accepting his terms
which are liberal. He will publish the report
on my name, pay all my expenses and \$2 a
day for two or three months of exploration
in the summer time. Before beginning the
comparative study of the coal plants of America,
I want to see the different basins and this
proposal of Mr. DeBow is a very acceptable one.
But for the books it is time enough to
procure them when they are wanted. And
if the academy of Boston can afford to lend
me those which I do not possess, I shall
keep quiet and do not want to buy any
more. -

I have put down as subscribers the
names of Mr. George Hunt Esq. and
Prof Chadbourne but since I have
only one copy left, I shall wait till
next week before forwarding their
orders. Prof. Hime's order has been
fulfilled immediately.

Judge E. Lane of Chicago, a
subscriber and Mr. James L. Bennett

Columbus O 12th March 1857.

Dear Sir.

You have my best thanks for the twenty Dollars for a copy of our Miniature for Mr. Ch. James Sprague. I shall ^{take} note the number of his Collection and send another copy for Mr. Blake. Perhaps you may have some orders for new sets and I can send all together. I will send a copy to Mr. Hunt of Providence next Monday.

Before yesterday (I think) I had the pleasure to write you a long letter. Now I am hurried by the preparation of the manuscript and beg you will excuse these few words.

With many and many thanks
your friend

A. Serquerey

Columbus, O. March 17th 1857.

Cher Monsieur

Avant hier j'ai reçu par Monsieur
Sullivant les vingt Dollars pour
l'exemplaire de M. Blake. Trois jours
après auparavant j'avais reçu le 20
Dollars pour celui de Monsieur Sprague
Et vous avais immédiatement accusé
réception. Aujourd'hui je prends la
liberté de vous envoyer par express
N^o 27 & N^o 28, l'un des exemplaires
pour M. de Candolle, l'autre pour
la première personne qui vous en
demandera un. Vous aurez bien
dans votre laboratoire une place où
le déposer en attendant. J'en envoie
deux pour éviter les frais de ports
L'autre parce que dès demain, je
quitterai Columbus pour quelques
semaines & peut-être pour plusieurs
mois. Cependant si vous m'écrivez
ou si vous demandez des exemplaires

des musées, les lettres seront
répondues immédiatement par quelqu'un
de mes fils & comme tous les exem-
plaires sont emballés, il sera facile
de les expédier de suite.

J'en ai envoyé un à Monsieur
George Hunt Providence & ai
écrit à M. le Prof. Chadbourn
pour savoir quand je dois lui envoyer
le sien. Les 25 nouveaux exemplaires
sont aussi beaux que les premiers.

En vous remerciant encore mille
fois de la bienveillance avec laquelle
vous soignez mes intérêts, je vous
prie d'offrir mes respectueux hommages
à Madame Gray & d'agréer
l'assurance de mon affectueux dévouement.
Les Lesguereux

Columbus O. 28th May 1857.

Prof. Asa Gray Cambridge.

Dear Sir. I returned to Columbus only yesterday and found at Mr. Sullivant a copy of your first lessons in Botany, that you had the kindness to send for me. I am truly very thankful for this new friendly present of yours and very glad that you have published this most usefull book. Though I have a heap of business to attend to, I have nevertheless read a good part of the book. It is perfectly clear, very interesting and contains all that is truly usefull for the study of botany. A most excellent book not only for the student but for the botanist who will want it every day for directions and reference.

I write to day to Mr. C. C. Frost of Brattleborough to offer him as a present a set of our Musc. caricati. He has procured us plenty of good specimens and is truly entitled to one. Since he is nearer to you than to Columbus, I write him to ask from you one set if he want one. Will you please have the kindness to send it to him when he has given you the necessary directions - Mr. De Candolle wanted to have one copy. But you sent one already. Neither Schimper nor Duby nor any one of our friends has received a copy of the Mosses of Sullivant. Did you not forward many copies to Europe following a list of Mr. S.? I have read at Dr. Owen's New-Harmony your memorandum in Sullivant's Journal

and have been very pleased and thankfull for it.
Our moses are selling pretty fast and I have only
six or seven copies left. I shall certainly not prepare
any more this year and for that reason would be
glad to keep for the future a few copies. - In one
month I shall go to Pennsylvania and perhaps to
the white Mts. If you are not gone from Cambridge
I might perhaps have the pleasure to pay you a
visit en passant

Most truly your friend

A. Lesquerer

Columbus O. 14th June 1857.

Dear Sir.

Looking over a package of mosses collected in Kentucky, I find a very fine *Loiseacanthion* which I do not ~~find~~ see described. When I collected it, I did not make much of it since I thought that I had seen described somewhere coming from Kentucky another *Dodecanthion* as the *Meistr's*. — This is somewhat hairy with crenulate leaves and grows on sandstone rocks in shady places below or in the fissures of the rocks. I found it only once on the way from the landing of Breckenridge coal Comp'y. (Ohio river) to the mines about half way up four miles from the river. Perhaps it is something new. It is the only species of phanerogamous plant that was worth collecting. But it was too early in the season when I was there.

I have a long letter from Godett. He is very sad about the final result of the much debated question for he is a true Royalist. I am sorry for him but on the contrary very glad that

my country may now last for better
days and for a quiet time. Godek says
he want to migrate with his family
to the Southern part of Russia and proba-
bly he will do it. But I truly hope
that he may change his mind.

Many thanks for your welcome
letter. Don't be in a hurry to sell
the musc. I have only 8 copies left
and they are wanted in Europe. If I
can go to the White Mts. I intend to prepare
next winter the 50 copies out of the
collected materials but only to be ready
and give my time to something else.
Smithsonian Institution has not sent
the copies of A. Sullivan's work to
Europe now. I have a letter from Schimper
and to day another from Mougeot who
are still waiting for it. It ^{may} have
been sent in the package of the
month of May.

M. Frost has received the Man-
and is pleased with them. He is an
excellent collector and his country is
rich in subalpine species.

Though always most happy to receive
your letters, I do not want you to
lose your time for my own grati-
fication. - Guyot stopped with me

one day this week, coming from Illinois
I was very happy to see him.

Please give my respects to Mrs
Gray. Most truly yours

Robt Serqueny

Thinking that I could perhaps go
to the White Mts about the 1st of July with
Mr. Jones of Philadelphia, I kept this
small package. Now I see that I have
no chance to find time and money
for the journey. For that reason I send
this plant and letter with a little for
Mr. Jones who will call at you next week.
Mr. Sullivan would much like it I could
go to the White Mts. But out of many
engagements, I think that the reason is
too backward for the White Mts at the
beginning of July. Perhaps I might go with
Mr. Foster of Lancaster in the beginning of
August. - Is this *Dodrantodon* a new
species? By comparison with specimens growing
in my garden, I find it more hairy, the leaves
less spatulate and more evidently petiolate,
crenulate and quite a different habit or
appearance. But of course I do not know
of the variability of this species and you may
think this only a small divergence of the
normal form.

Columbus O. 21th Sept. 1857.

Prof. Asa Gray. Cambridge.

Dear Sir. I am truly sorry to trouble you again about the package for Duby. This package contained 3 Urinasons of the fungi of Rarionel and I would much like to know if they are lost for I would then procure them again. You wrote me that they had been sent through the Smithsonian Institution and that you would make inquiries about them. A few words of you will settle the matter.

I have spent the whole summer in the coal basin, following the coal fields from Mississippi to their Eastern limits. This prevented my journey to the White Mts and my visit to you. I am truly sorry that I could not go. - Is this *Dodecantheon* from Kentucky a species or only a variety?

Most truly and respectfully your friend

Léo Desquerens

Coder can not forget the defeat of the Royalist party and says that he intend to emigrate with his family to Southern Russia. Did you hear from him. Has Mr. Hooker noticed the *Musci exsiccati* in his journal. -

Columbus O. 14^e Fevrier 1858

Cher Monsieur Gray.

J'ai reçu en son temps la somme que ^{\$180} vous m'aviez fait remettre par Monsieur Sullivant L'aum^e, dernièrement \$20 pour une ~~bonne~~ ^{bonne} plaie rendue à Boston, j'en avais. Comme j'ai donné reçu de ces valeurs à M^r Sullivant, je ne vous en ai rien écrit, mais je vous en suis très remercier de tout mon cœur de votre extrême bienveillance & de l'intérêt que vous portez à la vente de nos mousses. Je n'ai d'autre moyen de reconnaître ces bons offices que par une affection amère & dévouée.

M^r Duby m'écrit de Genève qu'il voudrait obtenir les brouillons publiés dernièrement de Lichens de M^r Tuckermann. Il a reçu en 1848 les 1^{er} & 2^e fascicules en un cahier; mais il n'a rien de plus. Avez-vous la bonté de me dire où réside maintenant M^r Tuckermann, ou quelle est son adresse & ce qu'il a publié depuis sa première fascicule. J'envierai immédiatement la valeur de ces brouillons nouvelles pour les obtenir. Combien vous reste-t-il de copies

ou plutôt deux exemplaires de nos musées.
J'ai beaucoup collecté et bien récolté de
collecter encore et et pour finir tout-
fait ce travail l'hiver prochain. J'ai
à vous remercier aussi pour une brochure
reçue la semaine ~~passée~~ ^{passée}. Son ^{je} trouve
des détails & des critiques fort intéressantes.
Le même je ne vous écris pas à chaque
fois, ne croyez pas que ce soit manque
reconnaissance pour votre bon souvenir.

J'ai rendu cette semaine tout mon herbier
de plante phanérogame 60-7 mille espèces
pour \$200. Il était entassé dans mon gilet
et les insectes le mangeaient à l'air. C'est
une bonne aubaine pour moi, maintenant
surtout. Je suis trop pauvre & trop faible
pour faire autre chose que les mousses &
peut-être les plantes fongiques. Cependant je
recueillirai toujours les plantes intéressantes
quand elles se trouveront.

J'espère que votre santé & celle de Madame
Gray sont parfaites comme votre bonheur
à tous les deux. Il y a de tout
mon cœur qu'il en soit ainsi. Veuillez
excuser je vous prie et importun tout
en être l'assurance de mon affectueux
de vouement. Les Serquereux.

Je n'entends rien dire de Godet, mais
j'ai eu une bonne longue visite de l'ami
Guyot en Janvier passé.

Columbus O. 19th March 1858.
Prof. Asa Gray. Cambridge Mass.
My dear Sir.

Your kind favour with a draft for \$20 was received to day. I am truly very much obliged to you for the trouble you have had with the sale of our Musci and I would be very happy if I was ever able to show you how deep is my gratitude for the kindness you have ever shown to me since my arrival to America. It is certainly to your praising of our Musci that they have been so readily sold. I have only three copies left and could sell them in Europe; but I want to keep them here till I find time to prepare the last fifty copies. —

M. Boissier has paid his set in Vaucluse. But I do not know if the Dillenert museum has paid in Europe or if perhaps the amount was included in the \$120 that I received from you through Mr. Sullivan. #1.
About two months ago, I took

#1 By looking over my notes I see that you have paid it yourself.

the liberty to write you about the lichens
of M. Tuckerman. Ber^t Duby of Geneva
has bought long time ago the two first
fascicles and he says he has heard that
there had been some others published and
he would want to have them. If you
ever write to Mr Sullivan please to give
him the address of M. Tuckerman, to whom
I may write about the matter. But
there is nothing pressing about it. On
Monday next I intend to leave Columbia and
will not be back before a few months.
I have been part year engaged in the state
geological survey of Kentucky for palaeontolo-
gical researches and I am called again this
year to continue the work. If I find some
interesting plants in my rambles, I will
collect them for you. I wrote you also in
my former letter that I had sold my whole
collection of phænogamous plants about 8000
specimens for \$200. As I have no time to look
at those plants, it was better to part with
them than to let the insects feed on them.
Don't you think so.

Please give my kindest regards to
Mr Gray and believe that I am most
truly & respectfully yours
Léo Lesqueren

Columbus O. Nov. 22^d 1858

Dr. Asa Gray Cambridge.
Dear Sir, Dr. Engelmann's copy of Musci
exsiccat has been waiting for him
since about two years. You wrote
then that he would call for it him-
self after his return from Europe.
As I have been absent nearly 8
months and returned home only
yesterday I could not of course
hear from him before. I have
sent the copy to day and you
paid for it when it was ordered.
It is all right.

On Monday next we will
take to the Japan mowers and
have these followed with Wright
mowers. As we intend to work hard
and work together with M. Talburt,
(and his Dravthman (spelling?)
we will be out at times.

I am now nearly fully occupied
by geological research and my own
business. I could collect nothing of
interest this year in phænogamous
plant.

Most respectfully yours
Léon Desquereux.

Columbus O. 2^d Dec^r 1858

Prof. Asa Gray

Dear Sir. Is there in the South of Florida or of Texas or of Mexico perhaps some species of Quercus that could be referred to both these fossil species sketched upon the paper. Fig. 2 & 3 is very narrow leaved. I have seen specimens still narrower than the figures. Fig 1 has very strong nerve and is allied to Quercus crassineria or Quercus gigas of Goppert (Tertiary flora). You will perhaps say that the identification of fossil leaves of phanogamous plants is impossible and therefore that it is useless to try to make a comparison. In the strata of somewhat different age, especially in the pliocene miocene and eocene strata along the Mississippi, the determination of the leaves, even approximative is truly important. For example the formation of which the accompanying leaves are taken is supposed contemporaneous with

the chalk banks of the borders of the Mississipi bottom which contain only plants of our time and still living in the same latitude. (Except *quercus virens* which abounds in the Oligocene formation below the mouth of the Ohio) And I would refer the formation with the sketched leaves to a older period.

I would deeply regret to have sold my herbarium if it had contained specimens of trees and shrubs of the South. I must now begin again and try to have such a collection or as many of specimens of leaves and fruits of trees and shrubs of South America as possible. I have still two copies of my *Musci Amer.* and would gladly exchange one for a number of specimens of shrubs and trees of the South. If you have some friend at New Orleans or in Texas, I would propose the exchange. I will also be much obliged for informations about this matter.

We are working hard with M. W. S. Sullivant on the Moths of U. S. Ex. of Japan &c: collected by Wight. They are very interesting but there is not many new species, I think.

H. Engelmann's copy has been sent.

Please excuse if I become trouble some by my inquiries. But I am well aware that you gladly help those who want a helping hand and that you have been especially always very kind and obliging to your old friend
I must respectfully
L. A. Lesquereux

one of them Heliconia occulta say is an extinct species. I have not seen any leaves in this formation and refer the position of the clay where they are found to a lower stratum. The identified leaves of that formation are very few. But I had scarcely time to examine it for a few moments and could only collect few specimens.

1. Quercus virens is abundant.
2. Gleditsia triacanthos (some leaflets) no doubt of it.
3. A piece of Acorus Calamus.
4. A Ceanothus which I refer with some doubt to Ceanothus Americanus.
5. Leaves referred to Glancra Ulmifolia also with some doubt. and 6th. another leaflet which I think is Gymnocladus Canadensis.

Further two nuts of Juglans oliviformis with a leaflet of the same. The nuts are certain, the leaflet is not.

The two species of Quercus which I sent you come from a different and lower formation which I refer to the ~~upper~~ lower glauca. The species which it contains are mostly new.

Columbus O. Jan'y 21th 1859.

Prof. Asa Gray Cambridge.
Dear Sir.

I am much obliged for your communication of oak leaves and shall be truly grateful for every species of trees that you can occasionally spare for me.

I will gladly tell you all I know about the new formation along the Ohio and Mississippi rivers. But it is not much. I had not time to examine them closely. The first deposit of leaves which I examined is below the mouth of the Cumberland river along the Ohio, Kentucky side. The leaves are mostly mixed with sand and alluvial mud and all belong to our time. At last I think so; I sent part of them three years ago to Heer and he found in these leaves especially Platanus acerifolia Willd. which he acknowledges as a variety of Platanus occidentalis and it contrary to Willd's now opinion accepts as a true American species. Somewhat later, about three years ago, I discovered a new and extensive bed of leaves below the mouth of the great Seneca, Virginia. This formation is peculiar. At some places the leaves are heaped, pressed ~~across~~

like a wall or a dam cut along the river by steps or stairs. At those places the leaves are nearly loose and can be easily separated from the sand and mud. At other places, the sand and mud that cover them has been hardened by carbonate of iron and the leaves are already destroyed but their print is well preserved in the strata. All this formation is recent. I would place it in the recent post-pluvial. All the leaves which I could determine belong to our species. Should you like to have these leaves with some transition of the formation, I can send them to you. Perhaps you could find something more than I can see. The formation becomes much more interesting near the mouth of the Ohio and below it on the Mississippi river. I must give you an approximate section of the chalk banks of Columbus Ky. that you may well understand the position of the leaves identified ^{here} below.

1. Beginning at the top and below the soil, a fine siliceous ^{grayish} loam, containing fresh water shells, not of the Ohio, but of the swamps, with plenty of fragments of crayfishes. 30 feet.

2. A conglomerate of hornstone gravel cemented by chalybeate waters. 30 feet.

3. Gray colored clay 15 feet.

4. White siliceous magnesian earths and indurated clay sometimes especially near the base as hard as limestone, 85 feet.

It is especially at the base of the clay that leaves, broken pieces of decayed wood and sometimes lignites are found. There is still below this a bed of whitish clay to the low water level of the Mississippi, but this last I have not seen, it and do not know what it contains. From the measuring of Dr D. D. Owen, the whole formation measures 160 feet. The composition and position of these beds varies much.

" At some points of the Ohio, the Hornstone gravel (says D. Owen) or conglomerate is replaced by a ferruginous sand charged with great abundance of shells, of broken fragments of lignites and of bones." In this ferruginous sand the bones of the Megalomys Jeffersoni were found. From my specimens, I examined the following species of shells. Melania canaliculata, M. undulata. M. acuta. Gelas similis & Paludine limosa. Near the mouth the upper part of these formations is absent and there is only a yellow siliceous earth containing shells. (of swamps)

geological age they belong. But as I
said, their analogy is with the plants of
the upper Miocene. - From the drift
I have seen nearly nothing. Deane and
Whittelsey sent me many years ago a
mixture of sand and broken carbonized
fragments of plants taken from a bank
of the drift near Cleveland. The plants
had been evidently transported and were
nearly in powder. I could identify a
single species and with doubt only very
needles of *Abies nigra*. - I have now
under examination a collection of fossil leaves
of the Miocene of Vancouver Island.

The species are most interesting by their
great analogy with those of the Miocene
of Europe especially of *Plurigen*. This ex-
amination is made for a Prof John Evans
of Wharfedale who intends to publish
the leaves in his report. I am just drawing
them and can write you more about
them if it is interesting to you.

In April next, I intend to stop a
few weeks along the banks of the Mis-
sissippi to collect specimens and study
the flora which to my mind is
just after the drift. I will also
go to the lower strata and to the
Miocene of Tennessee. Mrs truly

a most interesting study, and by and by
it may afford some data for the age and
the former distribution of some of our
species. - You could help me much by
securing for me the leaves of as many of
our hard species of leaves as possible, espe-
cially from the South. I know well
that you can't come to select specimens
and give them to me. But I would
gladly give one copy of my *Musci Amer.*
to a botanist of the South for leaves
and fruit of the southern species of trees and
bushes. If you could only now send
me one or two leaves of the following
species;

1. *Ceanothus* of the South. I have only
Ceanothus americanus.
2. *Planera ulmifolia*.
3. *Gymnocladus Canadensis*.
4. *Laurel Carolinensis*.
5. *Cladonia tinctoria* Raf. !!
6. *Andromeda Ligustrina*.
7. *Nyssa aquatica*.

you would truly oblige me much.
But if you have no time, do not care
about. We are going finally with the
Japan mosses. They are highly inter-
esting. Most truly yours

Leo Lesquereux,

In the plant of Safford (pliocene)
I have still identified Quercus myrtifolia
a Florida species. The identification is
certain. I am still in doubt about the
true place of the formation, but it is
certainly lower than the drift.

or do not belong to our time. They have
generally speaking great analogy with species
of the European Miocene or Pliocene. Of the
species of our time there is one leaf which
can be referred to Laurus carolinensis
+ Another to Cladrasia tinctoria Raf.
A third is (still with doubt) Andromeda
ligustrina, that is all. I could give you
conclusive answer about these three species
if you could send me for comparison a
few leaves of each. The only certain species
as far as it is possible to identify a fruit
is the fruit of Fragus ferruginea Michx.
I can not see any difference at all neither
in the form nor in the size of the fruit.
It is not emarginated at the top but point-
ed with the three sides bordered by a
narrow wing or margin and the base
somewhat ribbed. Out of these species
there is part of a leaf referable to Sapota
rubra minor & trees; a fine narrow
sally with entire margins; two Quercus
one of which is Quercus crassineura Göpp.
a type - (Q. aquatica ?? and a
Rhododendron whose analogy is still
unknown to me. These species were
found in Tennessee by Prof Safford of
Lebanon at whom I saw and studied the
specimen. He does not know to what

Columbus Oct 5th 59

My dear Gray

Dear Sir.

I was very pleased by your letter -
that I did not find anything new or even interest-
ing in Art? I am truly grateful for your
promise of securing for me foliage of trees and
shrubs of Cuba. I would I could get in exchange
for a copy of the Musci enciclopedia a collection of
leaves of trees and shrubs of the south.

No one of the Rogers has answered my letter.
As I know them, I supposed or expected nothing
else. H. D. Rogers at least has never answered a
letter except when he could get something for
himself or when he wanted to get it. This matter
has gone out of my mind entirely.

I have a good long letter from Gabot. He does
not know how the American life is reacting and
how ~~spreads~~ the leisure time that we can spare
for friendly intercourse. The difference in political
views has never existed between Gabot and myself
only he is still an enthusiast in something of

a fan he and Arner has on the contrary cooled my political enthusiasm to a temperature below ice. It is the only difference.

Nights music are still sleeping. I tell you I have no time now to examine them and I can not myself begin the examination before I have finished my reports on Kentucky and Arkansas. I would I could do it just now, but it is impossible. If we can begin at all this spring, we will go through. These notes are very interesting but very difficult to study. McCallum has not received the Tasmania money and probably, he says the parcel of H. Hooker is for him. McCallum wants a copy of the Cryptogame of Tasmania as you propose to get for him. He sends his kindest regards to you.

When I wrote you about the purchase of some books on forest plants and mentioned Witham (Structure of forest wood &c) you said that I could perhaps get the book from A. Sargent or from the Library of

Cambridge, at least for a short time. You have even a opportunity to get it either as a loan or from purchase. you will pay no service to send it to Columbia.

Most truly and respectfully yours
Geo. Sargent

therefore use at your convenience
my mention on this plants. But if you
do so please mention that the collection
was made by him. I do not know
to what academy I had better send
the paper. I am a fellow of the Society
of Natural History of Boston; but I think
that they publish only extracts.
Perhaps Philadelphia would be better.
I would like also a specimen of *Sequoia*
sempervirens if you have one.

Columbus O. Feb'y 10th 1859

Prof. Asa Gray

Dear Sir. I am much obliged to you for the
specimens. As often as I want something I will
take the liberty to ask you for it.

I do not remember well what I wrote you
in my last letter; I will then recapitulate the
leaves now identified.

1st In the post province of the Mississippi
which I now refer with some degree of certainty
to a formation anterior to the Gulf I have
identified. *Quercus virens*. - a *Ceanothus*
very nearly allied to *C. americanus* but differ-
ent - *Gleditsia brachanthos*. - *Acorus*
calamus. - and since I wrote you, from
comparison with good specimens: - *Prinos*
coriaceus or *integerrimus* - *Castanea*
pumila. - *Planera Gmelini* (from your
specimen).

From the lower province of Tennessee
I had for certain *Fraxinus ferruginea*
& *Quercus myrtifolia* and for doubtful
species. *Cladrastis tinctoria* and *Gymnocladus*
Canadensis. Both these ^{two} species
are more doubtful than ever now. They
don't agree with your specimens. But
the *Laurus Carolinensis* is certain for
another doubtful species and since I
wrote you I have still identified in
this formation *Bruna Caroliniana*!
There are many other species which I

would name with doubt, but it is better
to let them aside till further elucidation.
In Vancouver Tertiary there is a *Sabal*
which is probably one of our Southern species.
But the identification is impossible; there
are only fragments of leaves. I could not
even see the attachment and know of it
was *Chamarops* perhaps. One of the
Quercus is very near your *quercus multiflora*
and especially there is a fine specimen of
Sequoia sempervirens, the only species that
appears to be identical with the now living
species of Oreg. But the flora of the
Miocene of Vancouver is the same as that
of the Miocene of Switzerland viz an Equato-
rial flora. In fourteen or fifteen plants
enumerated, it has 1 *populus*, 1 *Salix*
with very large leaves, 5 *quercus*, 1 *Planer*
1 *Tilia*, 2 *Cinnamomum*, 1 *Persoonia*,
1 *Diospyros*, 1 *Sabal* and 1 *Salisburia*
This last species is remarkable. No *Salisbu-*
ria has ever been found as low as this.
The leaves are very polymorphous; oval
or elongated and undulate at the upper
margin; narrowed in a decurrent petiole.
I have figured and named all these
species for Dr. J. Evans of Washington.
I think he will publish them. But if
you want the report sooner he will
certainly lend it to you. But except

the mentioned species you would find nothing
of our time, I think. If you have many
Salisburia please to send me sometime
a few leaves of different species. Now
the report is ready and I can wait as
long as you like, or as long as you find
time.

We are progressing finely with the
Japan Mon. But it is a long work.
I will see Mr. Sullivant to morrow
and show him your letter.

Most truly yours

S. Lesquereux

I am very glad if you can use my
determination. I think you may rely to
those which are indicated as no doubtful
and do not care to ask for any. Think
you think worth having from me.
I have always time to write to you.

As fast as I can find or determine other
species I will send the name.

11th I did not close my letter yesterday supposing
that perhaps Mr. Sullivant would have
something to say. He will write himself
soon - I received this morning a letter
from Dr. John Evans about his fossil plants
of Vancouver. He wants me to publish
something in some Academic paper
before the report is ready. You may

Columbus O. March 8th 59

Prof. Asa Gray Cambridge

Dear Sir. I am much obliged for the leaves. The *Salisburia* of the Miocene of Oregon and Vancouver is true *Salisburia* by no narration at least. Though the outline is nearest. *Phyllocladus asplenifolius*. *Sequoia sempervirens* is right the same as the fossil plant.

For the fossil plants of the Pliocene and post Pliocene of the Mississippi, I did not make any drawing intending to visit again this spring the formations and places where they are found and to try to collect a larger number of them for a better examination. It will be time then to draw and publish them. Dr. J. Evans wanted me to make a mention of his plants with short description in the *Silliman's Journal*. I did so and sent at the same time a brief comparison of all the plants of our recent formations, at least of the plants that I have seen.

If Tillimans journal does not
want that paper I shall take the
liberty to send it to you for exa-
mination and you may then decide
if it is worth publishing or not.

I have a pretty large box to send
to Prof. Heer of Zurich. It contains
a set of our Musci Amer: and is
just of the same size only a little
broader as I have some specimens
to pack with. Can you tell me
what would be the cheapest way
to send it.

Most respectfully yours

Léon Lesquereux

Am 5
May 16

Columbus O 10th May 1859

Prof. Asa Gray. Cambridge.

Dear Sir Let me thank you for your kind letter of the 4th inst. just received. I left Columbus in the beginning of April on a business tour and returned home yesterday to spend one week with my family before taking the field. I had before leaving finished my part of the examination of the Lapon mosses. Mr. Sullivant whom I have seen this morning is not yet ready with the short diagnosis of our new species and as he is very busy with some business, he says that he cannot attend to the work before the summer months and that he will wait till August. —

I am truly glad that Dr. Hooker can send a copy of his fossil plants. As soon as you know what it cost, I will send the money with pleasure.

Prof. Heer to whom I sent a rough sketch of my drawing of the fossil plants of Oregon & Maine is delighted with the discovery of the plants of the tertiary of America and fully agree with me not only in the general conclusions but in the determination of the plants. He writes a very long and interesting letter which I will try to translate ~~it~~ to right and send to Dr. Dana for the Smithsonian Journal. Do not forget to read it, since it contains some facts according with the conclusions of the paper which was sent me with your letter. I am certainly most

alluded to you for the credit and praise you give to
my ~~leading~~ researches on the fossil plants.

This mentioning of my name by a Gentleman placed
in such high and general regard as you are was most welcome
to me especially just now. Rogers' report on the Tenn. survey
is finished and printed at least. I received a copy and opening
is found after my report. This unmerited rebuke from R.
"The foregoing essays by M. Serquey are the results of researches
made by him in the years 1852-54 upon the fossil flora of Tenn.,
under my direction, at the expense of the Commonwealth. I have
been therefore surprised and chagrined to find their publication
partially forestalled by the production of a portion of the same matter
in a report on the Fossils of the Western Coal Fields of Kentucky, recently
issued as a part of the Geological Survey of that State. In a prepara-
tory paragraph to that Report, M. Ser. remarks: "As for the
right I may have to quote a few lines of a report delivered in 1854
to the Director of the Geological State Survey of Tenn., I do not
think that it can be denied me. This Report elaborated with
great care and the arduous labor of two years, was to appear in the
Final Report of the Geological State Survey of Tenn. but it is a question
if it will ever be published. — In answer to this feeble attempt
at an apology for a breach of literary obligation, I have merely
to reply that the action of the Legislature to proceed with the
publication of my work was notorious, and that M. Serquey never
wrote to me to know if my well-known work had been disap-
pointed. — Now my report to Rogers contains about 50
pages 8° and 20 plates and the quoted paragraph contains a

single page in 8° and is referable to the characteristic flora
of the lowest bed of coal which is everywhere the same, as
well in Kentucky as in Ohio and in Penn. I could have omitted
the quotation; but Rogers would have had then a right to say
that I had exposed in my 14th report such facts as belonged to
his Tenn. report and which did not belong to me. As for the
publication of the Tenn. report, though I wrote to Prof. Rogers
about it, he would never answer to my inquiries. I learned
occasionally from his nephew whom I met in the Woods in Penn.
1857) that the report would be published in England and that
already my plates had been ~~part~~ engraved there. Immediately I
wrote to Prof. R. that according to his promise, I should have
the privilege of examining and correcting my report some ~~where~~
of my views were changed more than five years that it was delivered.
M. R. did not answer a single word but sent me by his brother
who reported me to return them immediately a copy of the plates
for correction. I did understand from this that I had to correct
the plates for the engraver, and returned them corrected. A few
weeks after Prof. Rogers wrote that the plates had been sent to
correct my report but that he could not part with the
manuscript for fear of losing it. As I had only memoranda
of my report and had to leave Columbus, I could not comply
with his request. I told him, must send an answer to his letter
asking him to write me if the report would be published in its
whole, if he would be published in my name and if I could obtain
fifty copies of it. Happily I have his letter, there is not a word
of answer about these questions. I stopped writing him only

and where I can't not but remark in my report since I was hired for palaeontological researches &c. in Kentucky

when he refused to answer. I wanted to know if he would deliver
me half of the specimens which I had collected and which were
repeatedly promised to me as well as the 30 copies of my report.
So this he would not answer and has never answered. I have
just received a letter of his best friend Prof Desor. It is
dated Montpelier 29 March 59. Desor has corresponded with
Prof Rogers ever since his return to Europe. He writes:
"Je suis curieux de voir son survey qui sera certainement jol
et bien fait. Mais para-t-il jama^{is}?" There is not
a single assistant of Rogers who till lately and after the
publication of my Kent's report supposed that the Ven-
survey should ever come to an end. Now, it seems to me very hard
to see a man for whom I have worked more than I would
do it for myself, attempting to take from me the only thing
that I possess in this world, the name of an honest man.
You will please excuse this long story, if you read it through.
My brother of D. B. would speak of
this to you or to somebody of your friends and I would not
wish to lose your friendship and confidence and the
good will that you have ever shown to me.

Most truly yours

Leo Lesquereux

1st May. From the paper you send me, it appears that just now
the letter of Prof Heer could be acceptable to you. I send you
therefore the translation of it and beg you will send it to Prof.
d. D. Lane, after the perusal or use of it, so that he may
have it printed in the next number of the journal. I write
him about it. It is translated nearly word for word but
if you read German and want the letter itself it can be
sent to you for Columbus. The letters addressed to me are
opened here. - You will easily understand what Heer says of the
Atlantic. He wants to show by the fossil plants that it existed between
N. Am. and W. Eur: an old continent of which the Canary island are
the remains. The coal flora shows that at the coal period this
connection between both continents did not exist or rather does
not show that it existed.

Can't you send me a copy of the paper and if possible of D. B. Lesquereux.

Columbus O. June 2nd 1859

Prof Asa Gray

Dear Sir. Your kind letter of May 15th is truly welcome. I find it here at my return from E. Kentucky where I have just spent my weeks to finish the geological exploration of the coal measures of that state.

I am now sorry that I did trouble you with complaints about Rogers' unjust accusations. I worried about them, it is true but I should have talked for myself alone a matter of no importance whatever and which has soon been forgotten.

Your letter contains leaves of *Oreocaphne californica*; I am much pleased to have specimens of it.

Your copy of botanical memoirs is also received. Many and many thanks for all your kindness which I can never repay. I have had not time till now to read the whole of your papers as I am just returned and am full of anxiety with my daughter's sickness. I have only seen that you quoted my researches on the fossil plants of recent formations and am truly obliged to you for mentioning my name. - The quaternary or sub or super glacial formation of the Mississippi extends itself all along the Ohio river and its affluents but much thinner than on the Mississippi. Examining it at Greensburg, above the mouth of Little Sandy, I found in it a fruit (seed) of the Japan and two nuts which would be valuable to *Juglans nigra* if they

were not smaller. I will compare them and report to you
as soon as I have time. As I have to go to Arkansas in
September, I will try to stop on the Mississippi for collecting
the leaves of the old and new species in plenty.

In my last journey in Kentucky, I did not find any
plant worth mentioning to you except a few specimens of
Cladrastis tinctoria Raf. along the river on the Virginia side
between Louisville and Warfield. A beautiful shrub indeed
but with a peculiar aspect, greatly contrasting with the
vegetation around it. There were no flowers.

I have not yet seen Mr. Sullivant. He, that he
could not give you a report on the Japan mosses.

Most truly your friend

J. Lesqueray

Croftumbus A. Jan 7 19th 1860

Prof Wm Gray Cambridge

Dear Sir

I spent Oct^r Nov^r and part of Dec^r in botanical and geological exploration of Arkansas, in connection with the Geological State Survey. Though the season was late, I have collected some interesting plants and made a catalogue of all the species which I have seen. I send the plants which are ^{certain} unknown to me, I take the liberty to send you the three following. - A leaf of Quercus lyrata Michx. As you do not mention it in the Is. flora perhaps it has not been found since Michaux. It grows in deep nearly inaccessible marshes near the Washita river. I could not find any acorns of this species. It is one of the largest and most beautiful trees of the Southern marshes. Its geographical distribution is somewhat more septentrional than that of the Chamaecyparis about 50 miles (in Ark^a). The second is Slip. Russine v. ? But if it is this species its range of distribution is not always along the coast. It grows on the tufa (a hill) formed by the carbonate of Lime of the Hot Springs of Ark^a, the only part where I have seen it with Samperus Virginica. The 3^d is an Utricularia which I do not find described anywhere and which is certainly different from U. pedatum by its nervature, ramification &c. It grows also in abundance around the hot Springs, generally hanging from wet rocks (tufa). But I had found it before coming to this place viz on the Western limits of Ark^a at the spring of a rivulet. I have plenty of specimens and better and larger ones than these. They are at your service - One of my

package of plants has been lost. It contained specimens of an oak which I supposed new. The tree is middle-sized about 30 feet high. Leaves somewhat like those of the *Q. tridentata* Eng. but wider at the base or at least emarginated by the stalk and divided at the upper part in two diverging points only; ^{grayish} ~~green~~ underneath. I have acorns of the species. I found it only a few trees, near the great springs on the limits of Missouri and Ark. - *Q. tridentata* is by far the most common form of *Q. nigra* in the Western States; the leaves being generally tridentate on the nearest branches to the ground with the normal form high up in the trees. This deeper division of the leaves of the oaks in ascending the tree is observed for all the species, I think. Would the catalogue of the plants seen in Ark. be of some interest to you, I would gladly send it to you at any time.

As I was directed to study especially the general distribution of the plants of Ark. in comparison with the geological nature of the soil and its agricultural properties (a comparison which scientifically speaking is nearly impossible) I have marked as much as possible the range of the most interesting species. Among them, the distribution of the Beech is truly remarkable. Along the drift sandy formation which form a parallel zone to the Ohio river, from the Wabash to the Mississippi river, it is entirely absent. This zone is about 30 miles broad. Nevertheless, all along the Wabash river, between the Ohio river, on ~~equatorial~~ or rather pleocene formation, ^{the Beech} is the essential component of the forests.

I saw a single tree of this species in the Mississippi bottom about 30 miles above the mouth of the Ohio. Hence the species entirely disappears at the carboniferous and silurian formation of Missouri and of Arkansas to reappear again in the greatest abundance in Arkansas as soon as the tertiary formation is reached. I followed it to the southern limits of Ark. and it descends certainly more to the southward. It grows there in the hills with the yellow and lobloby pine; in the marshy bottoms with the Taxodium; in the dry open wood, everywhere distinctly following the tertiary, even the cretaceous formations. This, I suppose, may explain the presence of its leaves and fruit in the red shales of the upper ^{of Pliocene & Miocene} ~~miocene~~ - - I would give you some other observations of the same nature if I was not afraid to take your time for nothing.

I read in Illiman's Journal that you are selling collections of Cuban Phanogamous plants collected by M. Wright. Could it be possible to have separately the trees and shrubs, or at least one leaf of each species. I would pay for them the price you think right. I am truly anxious to collect as many of the southern species of tree-leaves as it is possible, for comparison with the leaves of the tertiary and pleocene. I have got a great number of them and would like to study them with some chance of accuracy. The flora (trees & shrubs) of the Gulf is especially interesting for this comparison. The upper miocene of Mississippi (Steb.) contains a *Chamaecyparis* or rather a *Sabal* or two, also a *Sabinia* - a *Linnaea* - *monum* and some other interesting species. But most

of the species are still living. When it has deep opened
the var: without spines, now common now in the marshes
of Southern - Calif.

I have read with the greatest interest Dr. Hooten's Introdu-
tory Essay, and have ordered Darwin's book ^{to try} to understand
what may be the proof of this system of transformation.
My persuasion is opposed to it. But faith is of no account
in a scientific question.

Observe excuse this long letter. Most truly yours

Geo. Englemann

(A geological survey of Ohio is proposed to our
Legislature. I would much like to have charge of the
botanical department. (Paleontology and recent botany.)
Help? - except sufficient will push me but
as I am here without acquaintances, my chances
are very small indeed.

Columbus O. 28th March 1860.

Prof. A. A. N. S. N. S.

Dear Sir. When your most interesting and useful review of Darwin's book reached me, I was just receiving the book itself. Your memoir, resumming as it does the systematic propositions of the author was for me of great advantage. I wanted to write you immediately to thank you for your kind communication, but I thought that it was better to read Darwin's through to give you at the same time my impressions of this great work. Now, I can not find time for that purpose. I am just returned from Illinois where I spent two weeks in examining the fossil plants of the Geol. Survey of that State and must just now be in good order at home and get ready for a long absence. The State Surveys of Wisconsin, of Kentucky and of Arkansas offer me field work among the coal formations for at least six months.

Before Darwin's book came to hand, I had hurriedly prepared for Silliman's Journal a sequel to the 11th part of an article published last year on some questions concerning the coal formations &c. This I had treated the distribution of the coal plants, either geographically, both in Europe and America, or stratigraphically, on the different measures of our old fields. After reading Darwin, I have somewhat changed the plan of my remarks and I am now about ready to send the paper to the Journal. I will write to meet it to you, in the hope that you will find time to read it and to make the current calams the necessary corrections of style and language. Can I take that liberty? If it is possible, I would like to have it published in the next number of Silliman's Journal. I suppose it will take 20 to 30 pages. I must like the field next week, I will not be able to work over the

proof-sheets. I will try to write the liter. names so plainly that the printer can read them easily. I write you all this because I suppose that you have the direction of the Journal in Mr Dana's absence.

I have read to day the very interesting letter of Dr Newberry in the last number of the Journal. I am now sorry that I published Prof. Hear's letter about the fossil plants &c. I had entirely forgotten these two bitter discussions about Hear's statements and I was pained to see their responses by my fault. It is certain, at least I think so, that Mr Dana and Dr Newberry are right in their determinations of strata and fossil remains. I could never come to these fossil plants though I was anxious to get them for examination and thus I do not know anything about them.

Among Dr Newberry's ^{statements} statements he mentions some great mistakes. I have got now all the fossil plants of the tertiary of Illinois & Tennessee collected by Capt. Lafford and Graf Hilgaid, both state geologists. In some red shale which contains the foot of the Beach, have two species of *Ammonoites*, three *Harpagites*, three species of *Oriskanyites*, *Olea*, *Saururus* &c. three species, one *Carya*, one *Lalobura*. All plants that belong to the tertiary can not be referred to Cretaceous. They evidence by their general form a Southern flora than the ones inhabiting the same country at our time. Another series of specimens from S. Tennessee and from a somewhat lower position though evidently tertiary also, contains a beautiful *Crinoid*, a *Chamaeops* and at least one species of *Ammonoites*. This series is not now fully examined; but all the species of the other series are already figured and described; they can be published at my time. Where? Not in both the strata bearing fossil plants, there are (intermediate between the) *Ammonoites* strata ^{which} ~~with~~ abundant in marine shells. The upper tertiary fossiliferous stratum is found not below the chalk banks of the mouth of

the Ohio river, a formation which was referred by Dr Lyell to post-pliocene and which now from its fossil plants ought to be referred to the upper miocene. Then you see these miocene formations in Illinois Tennessee and all along the Mississippi river are contrary to Dr Newberry's opinions and statements. He has very well explored the Mexican plateau; but he does not know anything about our formations.

I feel every day more and more the want of better species of *Ammonoites* (lower and joints) I wrote to Mr Chapman of Florida, offering him a set of my Miami *Ammonoites* for a collection of Floridian *Ammonoites* species; but I did not get any answer. If I could make a living out of my business I think that I would move to Cambridge and settle there to have the privilege, with your kind permission of examining your collection. I am not so it, and perhaps it is better not to move my field work is useful for collecting materials which are most needed just now.

Darwin's theory is beautiful; but it is contradicted by the only data which could afford reliable evidence on the geological side. It will not do to put aside as useless the leaves of that great book, because they offer only till now detached and unconnected statements. We must read and admit as true what they say and can not argue of the possibility of existence of what they do not give any account. Concerning the philosophical deductions which may be drawn from Dr Darwin's system, I do not think that they can harm those who are checked by faith against harm. They may furnish some arguments to the materialists; but the unbelievers have always a store of them to my mind, it does not matter to the Christian naturalist whether Christus is, as I believe Him to be, a new creation, the first direct emanation from God of a new element completing humanity (incomprehensible) out of

Columbus O. 3^d April 1860.

Prof. Asa Gray Cambridge.

Dear Sir. As I have not received yet an answer to my letter I take the liberty to send you the continued part of the memoir on the coal measures. Probably you will not find time to read it. Therefore, as I have nobody here who could correct my errors of language, you would greatly oblige me by directing the printer to make those corrections. — Please, try at least to read the letter about Newberry's answer to Huxley. I am sorry to have to write it. You know that I would not notice any attack against me. But it is truly deplorable to see a true man of science and honourable in every point, injured for having taken the trouble to supply knowledge where it ~~was~~^{is} so badly wanted. If Newberry is right in his determination it is pretty clear that the questioned plants belong to the tertiary. But I think that he is mistaken and that they may thus be cretaceous. If nevertheless you find that the letter is useless, may give trouble to the journal by calling for an answer ^{or}: please destroy the letter and let the matter drop.

My views on palaeontological distribution with the data accompanying them can not be of interest in America. But European palaeontologists will certainly be pleased to know something of the distribution of our plants. I cut more than half of this memoir. It was becoming too long. ^{and I had no time} I start to-morrow for Indiana, but if you have the kindness to visit me, any time, your letter will be sent to me. Mr. Sullivant is somewhat unwell. He would ^{need} want to take

to field work and camping
Most truly your old friend

Geo Serquency

My package was at the express and
the manuscript directed to you when I
received your very kind letter. I took
out the manuscript and send it to
Prof B. Silliman, as you direct. But
I can not send this letter about
Newberry before you have read it
and give your approval to it. If
it does not meet your pleasure burnt
it. It does not matter a straw for
me if it is published or not. I think
I have to write it and that is all.

I can not tell you how grateful
I am for your friendly kindness to
me. Please to give also to Mrs
Gray my grateful acknowledgements
of your kind invitation with my
highest regards. I hope that you
are better and out of the influence.
Presumably Mr Sullivan suffers of
something like. I am too poor and
too busy to be rich. It is a solace
Most truly your friend

If the letter is ^{all} right please
to send it to the Journal. Geo Serquency

Columbus July 17th 1860

Prof Asa Gray
Dear Sir.

I have returned to Columbus and intend to stay here henceforth at least for this year. Silliman's Journal has published part of my paper on the distribution of the Coal plant. Probably you have had the kindness of correcting the proof. I am much obliged to you for the trouble. The printing is very correct, indeed. If the second part of the article is not already prepared and if you have not corrected it already, please to advise the Journal to send it to me for correction. I am ashamed to give you so much to do on my account.

I have to prepare for my report on Arkansas a Catalogue of the Living plants of that State. As it was already too late when I visited Arkansas last year, I could not see much of its Botany. Moreover I had to move very quickly and nearly without stopping except for the night, all the time of my exploration. Where shall I find

described or enumerated the plants of Arkansas.
I have Nuttall's Catalogue of N. Amer. plants.
and also Elliot, Darby and other Southern Floras
but I do not find much in those books.
Do you know something better?

I have read in Philist Journal Tansley's
on the origin of species &c. It is not much. What
he says is true. But his wine is diluted in too much
water. Agassiz's critical examination is better,
truly very good. But as Darwin admits it, Geological
Data are the only ones which can give authority
and furnish proofs. How are we to get at those
geological data, if as fast ^{as} they are exposed, their
truth and value is denied as it is by A. Wooster
in his introductory essay (not in Silliman's Journal
May, 60 p. 308). This is somewhat discouraging
perhaps. But after all, man is only trying
to come to the truth and the one only is wrong
who never try.

I have not seen Mr. Silliman yet. But
I know that he is well and working at the
Heart of the Icones. What a beautifull.

work he has done. He has certainly got the
head of Bryology.

Most respectfully your friend
S. Lesqueray

If you have something on the botany of
Arkansas and can send it, I will return it
after a few days.

specimens and shall continue to do so at
my leisure. And when I am ready I
shall try to go to Cambridge and spend
a few days with you for determination
of the whole. (This winter perhaps?)

Most respy your friend
Geo. Englemann

Don't return the specimens. But
only the names, if you please

Columbus O. August 23rd 1860

Prof. Asa Gray

Dear friend. I am already under many
obligation for your constant kindness for me.
I would have written you immediately after
reception of Hooker's fossil flora and the
pamphlet accompanying it to offer you
my best thanks. But I was then deeply
sunth in the examination of some plants
collected in Arkansas and I well supposed
that I could not go through without your
help. Now let me assure you first
that I am truly and much obliged for the
books and deeply regret that I am never
able to reciprocate your good offices in any
way. And secondly let me ask a new service
and pray you would examine, if you can
do it without any loss of time these few
plants and tell me the name.

Your great flora has helped me
much either for determination of my plants
or for mention of species found in Arkansas
by Nuttall. I never take in hand this

precious and admirable book - that
I do not deeply regret that it could
not be finished. The catalogue of the
Arkansas plants is not what I would
like to do it. But I am preparing
it as well as I can with the material
on hand and truly for a hurried
journey beginning on the 15th of October
and ended in Snow banks, I have noted
and consequently ~~found~~ seen a great deal
of plants. As I had no place for preserv-
ing specimens, I collected only what I
could not determine with you manual
when being on the field. I ~~had~~ ^{was} offered
an opportunity to go again this spring to
the south country along the St Francis
river of Arkansas, but preferred going
in the coal basin of Ill^o. Another
offered opportunity which I was still
more sorry to refuse was the exploration
of the tertiary of Miss^o. But perhaps
such good things may come again
next year.

The geological part (coal) of the
report of Arkansas is ready. Among the
fossil collected I found the wing of a
beautiful *Blattina* (cockroach) so very
near *Blatto Maderæ* that it can be
separated ^{only} with difficulty. I have figure
-ed both the living and fossil species.
This will go with some species of dung
against Mr Darwin. But from what I
read of the Discussions of Agassiz on the
subject, the more is said, the less we
know about the matter. But the road
to absolute truth is a long one. Among
the fossil plants of the true tertiary, got
from Miss^o there is beautiful specimen of
the great *Terminalia* figured by Sullat.
(I forget the name) At last it is no
possible to see any difference in the form
of the leaves, which form is truly remarkable.
There is also leaves of a *Magnolia* which
can not be distinguished from *M. acutifolia*.
Together with a *Hyamites* and a *Salicifera*
I have already begun to figure all these

Columbus O. Sep. 23 1860.

Prof. Wm a Gray Cambridge.

Dear friend.

You will have to cry enough! I am to trouble you again abusing perhaps of your friendship. After this, ^{little} I will try to keep quiet a while, I promise you.

Godet writes me that you are about sending him a package of plants and wants me to buy for him a copy of Chapman's flora of the Southern states. If you send a package, please to put with it the copy of this book and I will send you the money \$3 immediately. Godet can pay me at the banker who does our business at Vevey.

Another friend of mine, living here wants to know the price of the part (published) of your great flora, by Dr. S. I suppose he can get it at the publisher, but I would like to know what it cost. You remember perhaps that I once gave a copy to your great kindness and generosity, and thus do not know the price. You have read Newberry's answer. As it now concerns only myself I can not resent it and will let the matter drop. —

But I was truly disappointed and chagrined to see that Silliman's journal which had already cut in two my paper on the distribution of the coal plants of America has now postponed the publication of the second part the only one answering the attention of the introduction. Is this postponement due to Newberry's influence or to hostility to Marcon with whom I have nothing to do,

and with whom I am scarcely acquainted? In any case as I do not wish to be governed by any scientific party, I would rather have my manuscript returned than ~~to~~ have it accepted under any other influence but what it is worth. The publication of assertions which may have been anticipated might look ^{hereafter} ridiculous. As you are ~~now~~ continual intercourse with the directors of the Journal or are even one of the directors, you know probably all about it and I rely on your friendship to put the matter a right and to do what you think the best either having the manuscript returned or published if it is convenient. Last year I sent the amount of my subscription and it was returned to me with the kind remark of Prof. Dana that my contribution to the journal would be better than the money. It does not look as it was so.

Simplocos is generally called yellow wood in Arkansas and Louisiana. As I did not know this shrub and could not find the flower or the fruit, I asked some times the English name and got always the same. Of course many names are only local. In ~~my~~ the catalogue of the plants of Arkansas I took the liberty to follow your classification and English nomenclature thus calling *Simplocos* the sweet leaf. Of course you are the only reliable authority, everybody knows it and an unique English nomenclature is very desirable.

Godet writes a very good letter. He is happy and all goes well with and around him. I can not say as much for myself but perpetual sunshine can not do for this world. Most respectfully
your friend
Léon Seignureau

you think that the Academy of Cambridge would publish them in its memoirs. I wish to know it as soon as possible. These fossil plants of the tertiary mostly belong to Southern Geologists who have sent them for determination and if I have the possibility of publishing them altogether, I must ask their permission to do so. Some are intended for future reports. But they could be put in the reports hereafter all the same.

I have a separate copy for you of the papers on fossil plants published in the Journal. Do you wish one or two more for some friends? - M. Sullivant is working hard for Wright's money of Cuba. There is a few very fine new species. Schrader his draughtsman has learned engraving and will do pretty well without drawings.

Please to give my respects to Mrs. Gray. Very respectfully your friend

Geo. Desquereux

As the works on fossil flora are scarce and difficult to procure, my plan is to make drawings and descriptions of all the fossil plants of the coal of the United States even of the species already published by European authors. This would make at least one hundred quarto plates or more.

Columbus O. Dec. 26th 1860.

Prof. Asa Gray Cambridge

Dear friend

Christmas being passed, I cannot express a wish but the hope that you had yesterday a very happy time. Indeed I never think of you but with a sincere desire of your life being as perfectly happy as this world will allow. I know well enough that you have the same desire for me.

For the present, at least, I can not say that your desire is fulfilled. I have had lately or to say the truth, since the crisis of 1857 great trouble with my business. In partnership with my four sons, I had by and by, established an ~~important~~ house for watches &c. naturally I invested in this business all that I was worth and I think that all is lost. By the sacrifice of my house and every thing ^{that} I have, I will scarcely be able to pay my debts. I did not care much about this because as long as I lived I had the prospect of making a living by scientific exploration. But now the star of the future is darkened by a thick fog. I know

that it shines still, ^{above} ~~recovered~~ ~~under~~.
The wind of Providence will blow these ~~things~~ ^{things}
away in good time. Nevertheless, I can
not stop idle and I must try and help
myself. — Agassiz in his last letter
asked me what I intend to do with
my collection of fossil plants as he was
desirous to get it sometime for the Cambridge
Museum. This collection is very valuable
but I can not and will not part with
it till I have published the fossil flora
of the coal. This work is not progressing
much because I am at a loss to know
how I could find the money necessary to
advance the costs of this work. I know
I could get subscriptions enough to pay for
it by and by. But I can not rely on
subscriptions to publish it, of course.
And if I make a bargain with a publisher
I will or can get nothing for myself.
Don't you think that it would be possible
to bargain my collection to the museum
of Cambridge for a certain amount payable
for engraving the plates of fossil plants.
I would deliver the plants immediately
after the preparation of the plates and
if the amount to be paid by the
museum should be larger than

the value of the fossil, it could be returned
by the sale of the work. Indeed, I do not under-
stand very well what could be done in that
way. But if you ever had an opportunity
to speak with some person of influence at
Cambridge or also perhaps with Agassiz,
you could find the best way to help me
and to get something valuable for science
at the same time. The German report
can not give an idea of what I can do
now for the fossil flora. Both the reports
of Arkansas and of Illinois and also the
4th report of Kentucky ^{will show} of what extent my
researches have been till now and what
they are worth for practical geology.
Perhaps the fossil flora of the coal could
be published by the Smithsonian Institution.
But this work accompanied with a geolo-
gical and as complete (as complete as possible
now) of indication of the distribution of
the coal strata of the United States, may
be a sealable work and I would like to
keep a few crumbs for my old age.

Till I find something better to do,
I am now re-examining and drawing
the fossil plants of the Tertiary. I shall
have about 20 plates which will be
ready in two or three weeks. Do

Columbus C. 25 Oct 1861.

Wm. W. Gray Cambridge

Dear friend. A few days ago I received
from Mr. Sullivan the Review and the
Standard Writings (from Cuba) with
your examination of Darwin's theory.
Let me thank you heartily for these
communications, especially for the pleasure
of reading this last review. It is most
interesting, indeed and certainly the best
that has been written about this so
called new theory. I say so called, because
though I do my best to find it, I
can not see the difference between Dar-
win's and Lamarck's theories. The struggle
for life is a problem unconceivable for
me except by admitting it as an action
resulting from external circumstances, as
Lamarck will have it. But I am
probably somewhat obtuse in this
matter as in many others and in any
case it is no worth while discussing
my meaning just now. Let me assure

48
18
—
50

you that I have followed your explanation or examination pay ^{at} b. pay not only with the greatest interest - but with great pleasure. Your argumentation is deep, ingenious and witty.

But you know already the value of your contributions to science. Don't you?

It appears certain that the chalk banks of the Mississippi and all the strata containing fossil plants in Tennessee and Mississippi belong to different stages of the tertiary. Now, I have already two plates of these fossil plants and all except one *Sparganium* and one *Sabalum* belong to genera (and perhaps some of them to species) of our time and of our North America flora. There are ~~many~~ species of *Magnolia* with apparently the same characters as some species of our time. This extraordinary likeness contrast with the great difference of the European tertiary when compared with the ^{actual} flora of the same country. But

the comparison can be established mostly on the flora of Switzerland, ^{and} the position and topography of that country may explain the difference. I do not know when I can go to Cambridge. But if you wish to see the plates and compare them especially with living species, I can send them to you at any time.

I send you a report on Arkansas. It is just out. Perhaps the botanical table may interest you. I am perfectly satisfied of its insufficiency as a matter of science; but perhaps the plan might be followed for other catalogues of this kind and thus by comparison we could try and fix the geological relations of living plants, if there is any ???

Mr. Sullivant is well and works hard at the Cuban mosses.

Sincerely yours

Leó Lesquerens

and thus a mere material result as Lamarck will have it. Probably Prof Darwin will soon have more to say about A.

I have not seen M Sullivant for a few days. As I have nothing more to do now I have begun the preparation of plates for the fossil flora of the Coal. I shall try also to prepare the balance of my Musc. extincta, having still materials for about fifty copies. By and by perhaps time may be when such scientific works can become valuable. But now the horizon is very black indeed. When you write again please to tell me if you have heard lately from Godet? But do not write till you have plenty of time or need to write me.

Most sincerely your friend

Léon Lesqueren

Columbus O. 7th April 1861.

Prof. Asa Gray Cambridge.

Dear friend.

Your very kind letter of March 19th gave me the greatest pleasure, for there is certainly no man in this country whose opinion I more sincerely appreciate than yours. I would have answered your letter immediately if I was not always afraid to take for my own account your time always precious and too valuable for science. My answer to Prof Dawson is nothing but ~~what~~ I consider as truth. Of course I have seen more that the Prof. suppose because I have nothing else to do for science but go around and look at what interest me. M Dawson wrote me again. He says he understands my meaning better now. But I do not think he does because he appears to take my illustration of the peculiar growth of *Stigmaria ficoides* by *Sphaerium* &c: as if I would pretend that the plants of both these families were all the and had the same structure!

This is far from my mind. The answer which was sent to me by Prof Silliman has nothing whatever disagreeable to me. except ~~that~~ his assertions which are advocated as scientific truths and which I consider as being rather against it.

But Prof Dawson is answerable for his opinion and I do not see why it should not be published if he desires it. Only I do not wish to answer him because such scientific discussions become always a little too personal.

I do not know when I will come and visit you. Though nothing would give me a greater enjoyment than the spending of a few days with you and among your botanical riches. Of course I can do no much here, alone, without resources of any kind and I would have at Cambridge a great deal of precious advantages. But a blade of grass can not become a Magnolia or a Pine and I think that Providence has put me to vegetate just at the place where I am for the best.

Circumstances may change and show me another way perhaps.

No need to say anything about the Arkansas report such opuscula scarcely merit a mention. I only wish I could have spent a few months of the summer in Arkansas. It is a most interesting region for studying the natural distribution of plants and the phenomena that have influenced this distribution. If I was only rich! But perhaps if I was rich I would do nothing and thus all is for the best.

At first I thought, as you do that Darwin's evolution of species is not a mere materialist idea. But now I can not well reconcile the struggle for life with any Providential law of development. Is this struggle for life the result of an individual will or willingness? We can not admit it, I think. If not it is only an accidental need resulting from external causes.

Columbus O. Jan'y. 3^d 1862.

Most esteemed friend.

On Christmas Day, I got from our friend W. S. Sullivan a package of ferns which gave me the greatest pleasure, for I examined it the same day though I did not know what it was for. Your kind letter came here only before yesterday and I called immediately to Mr. Sullivan; but the package of money was gone some four days. Though I am afraid to give you the trouble to write me again, I want you to direct how I shall send the money. The package of course is all ready.

The 4th vol. of the Survey of Montg. has come out and I wish to send a few copies to Boston and Cambridge. One for you, another for Agassiz. and three for Boston. Could I not send to you all these copies with the money and perhaps you might

get the cost of the express from
the Gent. to whom the books are
addressed. I do not think that it
would cost more than 20¢ per vol-
ume. It cost 60¢ per. mail.

The ferns are beautiful many of
the species are unknown to me and
some extremely interesting for comparison.
I am much obliged to you for the
exchange. I have not got Prof
Paton paper on Wright's ferns. But
Mr. Sullivant has it and lent it to
me yesterday. I shall immediately file
the labels.

Your kind letter was very welcome.
I ought to be very hard for you to spend
your time in writing letters for the
pleasure of your friends. But what friend
of yours could be indifferent to a
kind remembrance of you.

Write only a few words about the
mosses as soon as you want them.

and I will send them in any way you
desire.

I was sorry to hear of Mr. Gray's
indifferent state of health and most
sincerely desire that she should soon
be well again.

Your old friend
J. Desquereux

Prof. Paton returned the ferns with
the names. I am much obliged for it.
But one herbarium was not with the
Desquereux ferns. It was found among
mosses and was probably overlooked
from its small size.

Prof. Asa Gray
Cambridge

Columbus O. 23th April 63

Prof Asa Gray Cambridge

My dear Prof

You would pay me a service to see if the mosses published in the last Botanische Zeitung by Muller are from California. A friend, Bolander, has sent me lately a large amt of fine species from Cal. and I have prepared a paper on them. Description of new species &c. which I desire to finish before leaving Columbus. Tuller, said to me about these mosses that you wrote him about Muller's publication in the Zeitung. But he does not know where the mosses are from. We expected to get Mr. Tuller's copy every day; but it does not come. If the mosses published by Muller are from Cal. and if you can lend me your copy of the Bot. Zeit. for one or two days I shall be much obliged.

With the mosses Bolander sent me a package of rhizogamous plants; grasses &c. This is useless to me. Shall I send it to you? This Mr Bolander is an excellent collector for mosses. I have already more than 60 species to add to the new series of the Musci Exotici.

It is a long time since I have heard
from you. I know I did not write you. But
you have always so much to do that I do not
wish to come to you and take even the
smallest part of your time only for my own
satisfaction. I am now out of Business,
a more than ruined man. But I hope to
at least make a living in some way. Providence
has kindly fed me for sixty years. It will
do the same a little while longer. May be
I shall have the pleasure to see you at Cambridge
this year. Gay, Agassiz and the Smithsonian
institution intend to send me around for
collecting fossil plants for their collection.
If I go East I must go to Cambridge for Agassiz
fossil plants &c. I hope you have been
well and prosperous all the time

Your old friend

L. Senguer

Please to remember me most respectfully
to Mrs Gray. I hope she is now quite well.

Columbus Feb 21th 1864

Prof. Aza Gray. Cambridge.

My dear Prof.

As soon as I got your letter of Feb 4th. I wrote Schimper about the masses of Cuban. I do not doubt that he is still desiring or anxious to have them, but it may be that he wants more than two sets and I would like to have a positive answer on the matter. Nevertheless, if your package is ready before I get his letter, I think that it would be better to forward to him the two sets positively demanded in his letter of next year.

I did not answer your kind letter immediately because I wanted to see if perhaps I could be ready with the preparation of the second edition of the music at time to send with your package a few copies of the work to Europe, especially to Schimper. But I see that I can not be thorough before three months at least. I have still the whole series of the *Oleuroscapi* to prepare and so much to do besides the work.

I thank you sincerely for your kind letter and the determination of the fees. I showed your letter to M. Sullivan who is well now, at least well enough to be out and moving. That building

29th

for your botanical treasures will be a fine one. I hope you will enjoy your botanical life still for a very long time but the disposition you are making now of riches of such high scientific value and obtained by such a hard and constant work is most noble and commendable. Our botanical posterity will bless you for this world.

You do not say any thing of Mrs Gray's health. I would be sincerely rejoiced to hear that she is now quite well. I am give her my sincere respects.

It is a long time that I have heard nothing from Gadet. Boissier writes that he would like me to go to California for collecting plants. My career as a collector is over, I think, and certainly Bolander who is there yet will do far better than I could. Nevertheless if I had a good opportunity to cross the plains and go to California, stopping one year or so with Buckley Mr. I could scarcely resist the temptation of doing the thing I have dreamed of California time and again.

Very sincerely your friend.

Leo Lesquereux

Columbus O. April 23rd / 65
Dear Prof Gray.

M. Boissier of Geneva in a letter received yesterday says: - "Si vous écrivez à notre ami Asa Gray, veuillez lui présenter mes compliments affectueux et lui demander quand seront prêtes pour distribution les nouvelles collections de Cuba et les brochures qu'il a promis de m'envoyer." - If I remember well what you wrote me, all this was forwarded to M. Boissier with Atolls plants. By this time he has certainly got them. He sends a package of plants to Hall and I will have to acknowledge the receipt as soon as Hall has got it. Have you perhaps something particular to answer to his note copied above.

Bolander demands the names of some mosses which, if I understand right, he says he sent in a package of yours. Have you ever got a small package of mosses for us (I received one through M. Sullivant, coming from you, I think, in July past. But it contains only doublet specimens for the Musci, all without labels and Bolander speaks of specimens with N^{os}.

yesterday we received part of the labels. They are just at time and I will begin at once the preparation of the sets. You have had much trouble about that matter. I am sorry for it and thankful for your kind office in that matter.

Would it be possible to buy separately N^o 29 vol. VIII. June 20 of the Journal

of the Proceedings of the Linnean Society (London)
It contains Mitten's paper on Californic mosses
I need it very often for reference. If you can
get it for me I will send the money as
soon as bill is received.

I hope that you are quite well and
Mrs. Grays health is improved. Please give
her my sincere respects. I received your
paper on the Botanical museum of Cambridge
and read it with great pleasure. Though
I do not always acknowledge your scientific
communications for fear of giving you some
useless trouble; you well know that I always
receive them with great pleasure and read
them with great interest and profit.

I do not know if I ever wrote you about
that pamphlet on Californian mosses. It would
have been far better if M. Sullivant had prepared
it. But he had no time and as Bolander
sent at the same time the same mosses to
Hampe and to myself, M. Sullivant urged
me to examine them and report on them
in order that priority of nomenclature should
be preserved for us. Hampe had moreover
to hastily report on a few mosses sent to him
from California that it was desirable to give
a better account of his species. My ms. was
submitted to M. Sullivant and approved by
him and though Mitten does not like the
diagnoses of some species of mine which he
has also published or reports, they are as satisfac-
tory and as reliable as some of his own. I do
not know if Californic Thallozoum plants
show the same extraordinary disposition to

variability as the mosses, which from this
reason are extremely difficult to describe.

Very respectfully your old friend

S. Lesquereux

#1. I am
satisfied that the sets contain more than two thousand specimens (no you). I am
in need of 5 to 10 specimens representing different forms of the same species (capital) having

you suppose that the price is too high. If we should count what each set has cost us either in time or money it would certainly be more than \$1000 for each.

I do not write this long letter as a matter of interest to you. But you may have (and I hope you will) some thing to say in the Journal concerning this work of ours and may be you will find some materials in this detailed account.

I will forward the package as soon as your answer is received.

Very sincerely your friend
L. Lesquereux

I have offered my respectful regards to Mr. Gray and at the same time my sincere wishes for the restoration of her health.

I have been to Mr. Sullivan's time writing the above. He supposed \$35. would be somewhat high. But per contra Austin Lugo Clinton and others say the price is too low. #1 I think that if you should mention that both the London and the Paris museum have subscribed for the work, many here would follow the example. Do you send something to Bolander. I will send him one set free of expenses of course but how?

Columbus O. March 19th 1866.

Prof. Aza Gray Cambridge.
My dear Friend.

At least the second Ed. of the Musci exsiccati is out and the sets packed and ready for distribution. You ordered a set for the British Museum. Shall I forward it to you per express with your own set. I may have in one month a big package to send to Schimper and another to M. Boissier; but it would cost as much to send to England from Strasbourg or Geneva as from Cambridge, perhaps? If you think that you have or may have the placement of some other copies, I will send you any number, as far as I have them.

The number of species and good varieties of this 2^d Ed. of the Musci is 536. The work is indeed quite a new one. The labels have been reprinted to allow an uninterrupted series and the placement of all the species

according to their scientific relation.

The collection is enriched by the species of California, admirably collected by Bolander ^{and a few by Prof. Brewer} and by some of the Rocky Mountain communicated by Hall. The number of Eastern species has been also increased by the researches of James, Peck, Austin, Sarge Clinton, Engstrom and myself and the specimens of the old species have been either renewed or increased as much as possible, in number and in scientific value. Of course a few species are represented by specimens which are not quite satisfactory and complete but these species are few and we found on purpose to give them, as they are, to complete as much as possible the representation of our Bryological flora at this time. And moreover, there is not a specimen of the whole collection which can not be used with advantage for comparison.

The preparation of these sets has been

a very long and trouble some work. Not only for the preparation and determination of the specimens which it contains but by the constant call for microscopical examination of an immense number of specimens sent for determination by some contributors. But this has had a good result, encouraging some young botanists to the study of the mosses and cryptogamous plants and thus causing the discovery of a certain number of yet undiscovered species. Our bryological flora is ~~thus~~ becoming somewhat appreciable in its richness. Nevertheless I do not believe that if we counted the whole extent of the U. S. from one Ocean to the other, more than one half of the flora is ^{yet} known.

About 10 copies of the music will be distributed gratuitously; the balance of the sets will be sold if possible. What do you think of the price. I purpose to sell them at \$35⁰⁰ per set in America and \$45⁰⁰ in Europe. Do

Columbus O. March 28th/66
Prof Asa Gray
Cambridge

Dear friend. Your kind letter
concerning the price of the teeth is most
satisfactory. We agree with Mr Sells.
-want that the price is \$35⁰⁰ in gold.
Please to announce it in that way.
I have delivered to the Am. Express
this morning one Package to your
address, containing your own set
and that of The Museum of London.
As you say nothing for Paris, I
suppose if they want one there,
Schimper to whom I will send 5 sets
(as ordered by him) may place one
there. All those to whom I have
sent sets, Austin, Beck &c. write
me that the price is too low
and that the world is above every
kind of praise. Mr. Peck says that
it has opened a new world to him.
The Museum of Chicago and that of

Albany have sent orders for one
set each. I will thus have some
well come remuneration for my work.
Mr Sullivan will accept nothing for
his expenses which are entirely gratui-
tous. He paid the printing, you
know.

I send a receipt for £7 Sterling.
But if you have at London some
friend whom you may trouble with
a commission for me, please demand
of one of them to buy for me with
part of the money a copy of the
fossil flora of Lindley and Hutton.
This work was in my hands for a
time but the proprietor sold it
and now I often want to consult it.
I suppose that \$15 to \$20 or less
may be the price. It is not any
more in the trade but has to be
bought from old book stores.

Allow me to thank you again.

for your kindness in this matter
as in many other.

Your sincere friend
S. Desguereux

See that you do not pay the
Express charges. They were paid here.
The set for England is a very good
one, yours also. But indeed they
are about all alike. If you can
spare some copies of your memoran-
dum of the Musc in Silliman's
Journal, please let me have them.

to pay her a short visit before
leaving your house. How pleased I
was to find her the same most
amiable and charming lady as
I had seen her years ago. Please
to offer her my most respectful
regards.

Your friend very
respectfully

A. Lesquerens

Had you not better send
to M. Sullivant that package
of mosses from Cuba and write
him that Wright is anxious to
have his determinations. Nobody
could stimulate him with better
results than you. I think that
Prof. Agassiz will send me some
books. You could put the
package with.

Columbus 14th 1867.

My dear Prof. Gray.

Do not scold me for having left
Cambridge without paying you a third
visit. I wanted badly to see you again
if it was only to thank you for your
great kindness toward your dear friend.
But the weather was very bad, I was
anxious to leave in time to reach Colum-
bus on Saturday night the 2^d Nov. and
indeed I supposed that I had taken
already too much of your time and that
you would be thankful to be spared
one of my boring visits. - I wish
that you had given me that package
of Cuban mosses for M. Sullivant
whom I have called and with whom
I spoke about that supplement for
the manual and about the Cuban
mosses. He does not like the idea
of helping in the publication of the
whole series of the Cryptogamous plants.

He says that he would rather make an independent Synopsis muscorum with not very long descriptions but at least 2 or 3 times as long as they are in his manual. But before coming to this, he would first do the Cuban and Venezuela mores which he has not begun yet. His conclusion were: that he would think it all over and talk with me about in a week or two. I believe that by and by Mr. Sullivant will and must begin to study mores as of old. Now that his house is built, he has nothing to do and a country life without scientific entertainment should become tedious to a man of such a mind as he has. The mores of Alabama collected by a friend of mine living at Mobile are of great interest. Many are Cuban species. I sent one to Mr. Sullivant with the

request to see if it was not *Trichostom macrostegium*, one of his species. He was pleased indeed to look at it and he said that when I was ready we should take a look at them. In that way, I will try to stimulate him and take him out of his *Papraea* which, if I do not mistake is his beautiful wife. The set of the Cuban mores is of the greatest advantage to me. But I can not speak to Mr. Sullivant of my wish of examining the packages of Mr. Wright. If he undertakes it, he can do the work far better than I could. But I offered him to help in the work as much as I should be able to do it.

Now I thank you again and again for your kindness. I had a delightful time in examining Martins and far more delightful still in the company of Mr. Gray who allowed

Columbus O. Feby 6th 70

Dear Prof. Gray.

All your friends before now have sent you words of welcome ~~now~~. I can to believe that the last one coming now to give you the hand of welcome is the most sincere of all. I hope that your European Journey has done you the greatest good and afforded you pleasure and advantage and that it did this same for kind and most amiable Mr. Grey.

I got lately a good letter from Mr. Boissier. He says among other things that he has deeply regretted to miss your visit at Geneva, he being then in Italy for his health and in great danger of losing his life. He sends me his photograph and says that probably Mr. A. S. Candolle has given you one of his ^{cards} for me. If it is so please to send it and do not wait till you have time to write for after your long absence, you have probably more to do than ever.

My health has been very poor the whole of past year and I have still had hard trials in other ways. But

my eyesight which was nearly lost is
improving and all will be well to
the end.

Please to offer my most respectfull
regards to Mrs. Gray. Your friend
very truly A. Sengueren

What is Fargalve urens for a name
of this kind, which I find attached to
a fragment of leaves given to me by
a gardener. If you do not know it,
please do not lose time in looking for
it. That fragment is palm like, a
triangular leaf with a long beak-like point.
The plant is cultivated in European gardens.

Cambridge 13 Nov^r 71.

Dear Prof Gray.

You have my best thanks
for your too kind note. I will
call at five O.C. P.M. to day
for a short visit as I can not accept
your invitation for dinner. Please
do not stay at home for me if you
are called elsewhere as I will call
every day at the same time till I
find you disengaged.

Your old friend very respy
S. Lesquereux

Columbus O. Oct^r. 12th 72.
Prof Asa Gray Cambridge.

My dear Professor.

Many thanks for the communication of a copy of your admirable Address.
~~I do not think~~ that I ever read any thing of this kind with more pleasure. Not merely because I find in your statements an encouragement to my modest researches from such a high authority as yours; but because what you say is beautifully expressed, true in every word, deeply considered and far sighted in the suggested or derived conclusions. It is a most valuable contribution to the advancement of Science. I believe that your statements, even those which you present hypothetically will be more and more confirmed by the study of the tertiary flora of ours, especially when we are able to compare both the Eastern and Western tertiary & Cretaceous plants. — My tour of exploration from the Plator M^ts., New Mexico, along the base

of the M^t to Cheyenne and along the
U. P. Rail Road from Cheyenne to Salt Lake
was of great interest to me; not only by the
large collections of specimens of forest plants
which I made, but by examination of
the localities for comparison of geological
stations &c. I should also have got a good
many specimens of living plants if I had
had time and mind for them. But I
can not attend to two things at the same
time and I could scarcely come out of the
geological researches which required as much
eyes and attention as I could spare. I
got only specimens of some shrubs & trees.
Of the scenery I saw nothing worth admiring.
What an immense difference is there between
these Rocky so well named, and our
Swiss alpine or even Jurassic Mountains.

You got certainly more rosate impressions
from your tour, especially from the Yosemite
Valley. I hope that both Mr. Gray and
yourself have returned highly pleased
and especially fully restored in health.

Please do tell me what is this Acer of
which I send a leaf. I got it in Ogden Canon
above Salt Lake where it grows in company
with Acer glabrum & A. tripartitum. It
looks much like A. campense but is vit-
=lous on the lower surface. I could not
find it with fruits. And also what is
this Oak growing at the base of the
M^t near Canon city Colorado with that
Quercus referred by Watson to Q. alba,
(an identity which I can not well see). It
is Q. douglasii of Hall from specimen.
But this one is evidently different. It
is named there Colorado scrub oak; but
perhaps my informant mistook it for the
other species which I think bears the same
name. I have only one branch with
fruit and will send it if you desire.
Do not return specimens.

Most respectfully and truly yours
S. L. Engelman

Columbo 0 April 30th 73

My dear Prof

Our good friend Sullivant
died this morning. Since the first
attack of his disease he never rallied
but slowly declined without great suffering
to the last. I never saw him for three
months. A great loss. He was here
my only friend.

Yours most truly
A. Sengueren

12th This note was put aside as
not worth sending. But to day I have
definitively accepted to go as a member
of the scientific expedition now fitting
by the Smithsonian Institution for the
high waters of the M^o. & Yellow Stone.
I may have to be soon on the way.
If therefore you have some direction
to give to me either for Sullivan's mosses
or for any other matter please writ me
as soon as convenient. - This under-
standing is a hard one for my old
bones. But I shall try to follow to the
end (the end of my life) the work begun
on American vegetable paleontology and
thus explore as long as I am able to go
Whitney's plants will be seen through I
think before I leave.

(Lumber 10th May 73.

My dear Prof Gray.
The loss of our most kind friend
Sullivan has quite unmanned me. A true
good friend is the nearest relative we have.
I could not write you before now but sent
you the papers with some account of him.
For you want to write something about him
for the Journal and some details of publication
of his may be acceptable to you. - Did he
write you on the publication of the 2^d Vol.
of the Iones? This vol. should be published.
The plates are splendid and all ready and
with Sullivan's notes, the descriptions can
be made easily. Of course he has given
you his herbarium (bryology) at least
he told me many times that it should
go to your cabinet after his death and
there is no more appropriate place for it.
If I can be of some use here in any way
please say so. Since I have left the
mosses and the Microscops, my relations

with Sullivant have been less frequent but not less intimate. He was pleased to show me every thing about his work, preparations, plates &c. His cabinet of anatomical preparations of mosses is very valuable. It is mostly mounted for the Genus Othotrichum; but this is the most difficult of all. — I am told that you are sick but hope that it is not true. However you would too hard take care of yourself. Of the strong columns, you are the only one left. What could botanical science do without you. I see that Orsbisson too is dead. He was like Senecio — a friend of old; what a pity to be left alone on the road. —

I do not think going to Cambridge this year, I have too much to do at home. ^{But} Now I am examining Prof Whitney's fossil plants from the Cliffs of California; a most interesting study.

~~They~~ have the general character both of the old tertiary plants and of those of our time and what is remarkable and what I do not understand yet, these types are more like our Oriental ones or nearer to our present species than to those of Japan. There is however a splendid species of Arabis much like a species which I saw in your garden marked Arabis papyracea of Japan. Is this name right? I do not find it in the prodomus.

Of course I write you to have a talk about our lamented friend. If you are not well and too if you have no time, do not trouble you to write me.

Your friend very truly
A. S. T. J. J. J.

After Whitney is at Cambridge and if you see him please tell him that I shall send him an account of his fossil plants in about two weeks.

Columbus 0 15th May 73

Swf Aza Gray Cambridge

My dear friend

I met yesterday at Sullivan's room the administrators of his estate, his wife and son-in-law who wanted me to see and say what should be done with the part ^{of property} which has not been devised by his testament. It is all his notes with all the books in his own room & the biological works. I do not understand why our friend did say nothing about it. The administrators considered with my advice that all should go to your herbarium. Mr Sullivan has told me many times that you should have the whole and there is no place where such a precious collection could be preserved and rendered useful but at your cabinet. The question is: 1st Would you accept the gift? 2^d Is it appropriate to send it now. If the musci or rather Scenes 2^d vol. have to be published, I shall have to prepare ^{perhaps} descriptions with the materials left

By our friend. Part of these materials,
already full descriptions, are in a separate
book where he began the work; but most
of them are upon separate leaves joined to
sketches and specimens in his herbarium.
Therefore if the collection is sent to you now
the work of description &c. can be made only
at Cambridge. On another side, Mrs. Pitt.
has already sold her house and the Rooms
must be emptied before the 1st of July. My
own house is so small that I have scarcely
place enough for my own books and cabinet
and it would be impossible for me to store
even a small part of this valuable property.
As Mr. Sullivant was president of the Medical
college to which he has given all his apparatus
microscopes &c. probably the college would lend
a room till the work on the Iscues can be done.
When that will be, I can not see. I have
to go to the Rocky Mt. again for geological
explorations and when returned, if I return,
I shall have work enough to do for reports &c.
and thus the work of the Iscues may be
indefinitely postponed. Could you find

a way to arrange matter and advise what
should be done for the best. The plates are
engraved but not printed. They belong to
Mr. Sullivant's estate of course. I could not
learn as yet if they had decided to pay
for the printing and other costs of publica-
tion. I wanted to hear from you before
advising about the matter. If you can
tell, of course you can not visit me but
perhaps you can direct somebody to write
a few words about the essential points: disposition
of the collections &c. and especially about
Gaylord's collections which belong to Mr. Somell
as far as I know. I wish to know to
whom address them. I may have them
brought to my office and packed as
soon as I know to whom I shall direct
package or box.

Yours very truly,

L. Lesquereux

What is the cost of the Brazilian flora.
The agricultural college has got the botanical
books and they want to know what it will
cost to continue subscription to that flora

and of the parcels which belong to the
Harvard collection. I then took out the
packages of Taylor's Herbarium which
McSullivan's brother promised to send.
Among these packages there is a small
one which from Mr. H. annotation, belongs
to Mitten and should be returned to
Prof. Hooker at your first convenience.

I do not know as yet when I shall
begin work on the Icons. The plates are
not yet received or printed. And, as will
you know, I have to live by my work
and thus to accept work as it comes
and as it pays. This I say merely in account
of former engagements which I can not
put aside just as I may desire it or
which have to be fulfilled in a fixed time.

The materials of McSullivan's Herbarium are
most valuable. Besides his personal collection
there is a large number of packages still
unopened and undetermined.

Ever the same L. D. Sargent

Columbus O. May 20th 73
Prof. Aza Gray. Cambridge Mass.

My dear Prof.

Yours of the 17th came this
morning. I have been to Mr. Sullivan and gave
it to him. He says after reading it that he
has yesterday rented a Room in the Medical College
for the Biological collections and Mones of your
Brother and that he wishes to keep the whole
here till the Icons and Manual are ready
and that then the collection is to go to
Harvard with the books of course. As I
do not think to go to that war expedition
Leidy and Sexter having backed out and
myself having accepted only with great reluctance
and conditionally to their being members of the
corps. I can do nothing without good and
friendly assistance and I have therefore demands
to be released of such an engagement which
however was only half ^{taken} ~~accepted~~. I shall therefore
have time to soon prepare the work a more.
My purpose is to have the descriptions off

the Scores prepared here, then the mss.
sent with a copy of the plates to Schimper
and to Lindenberg for corrections and
remarks and the whole published in
W. J. Sullivan's name only as a memorial
of his three best friends in ^{Botany} ~~botany~~. While
this is done I will prepare the Manual
of which the plan has been made long time
ago both by W. Sullivan and myself or
for a common work. It ought to be published
in both names as he wanted it and should
appear immediately after or even with the
Scores in order that nothing of his can
be appropriated by others. If Schimper or
Lindenberg assent to that, they may too
have the privilege of making additions
and corrections. I hope that this
will be satisfactory to you. As soon as the
plates are ready I will begin the work
if I am here and I shall try to stay most
of the time at Columbus this summer
in order to push the work along as

well as possible. As soon as W. Sullivan
has time I shall go to the bloom, make a
list of all the books packages which
will be sent to you as proprietor of the
whole. I will at the same time return
to you Taylor's collection.

Your most sincerely
S. L. Serquency

June 7th.

You see by this letter that I
do not even dare to send you my letters
after writing them by fear of being
troublesome, or of uselessly taking your
time for my own advantage. Mr. W. J.
Sullivan told me that he would
write you all about his arrangements
for his brother's botanical property and
therefore I considered my letter as useless.
I have been still once in W. Sullivan's
room and have finished invoice of
the botanical library or of the book

of his loss without deep regrets though
old he was. His whole life has been
devoted to good deeds.

Your old friend

A. S. Lezueux,

Columbus O. 10th Dec^r. 73

My dear Prof Gray

Accept please my sincere thanks
for the kind communication of your last
paper: Character of Gen. General and Notes on
Compositae etc. All what you write is so valuable
and interesting that I get from its study
a great deal of profit and much pleasure too.

Sullivant as you know from his brother's
letter, has left a package of Venezuela moths
which he gave to Schrader for preparing etc
with them on condition that Schrader should
make good drawings of each species and give
him a very good set of these moths accompanying
the sketches. Now Schrader claims these drawings
for the reason that he can do nothing with
the moths which are not determined, Sullivant
having never had time to attend to that. Schrader
came to me yesterday and I proposed him to
send Sullivants set with the drawings to
Lindberg requesting him to publish the
species with description and so? as it was
done for the Cuban moths. Schrader should

then have a number of copies of the pamphlets corresponding with the number of his sets and Sendberg would have a copy or a set for his trouble and return Sullivan's set and drawings which could therefore be preserved in his collection. What do you think of this arrangement to which I believe Sendb. would agree. I shall write him just as soon as I have your answer, having to send him my copy of the Cuban money which he is anxious to have for a short time.

I am more than half through with the Supplement of the Icones. You know that Sullivan was extremely careful not only in his descriptions but too in the preparation of the Ms which he had copied by some good hand at writing. I suppose however that the printer can read my mss. without having it copied. There are of course some erasures and additions etc. but the mss. is much better ^{written} than are those which I generally send to the printer. Is it necessary to have it recopied?? Will you have time to read

the proof sheets, at least once. For of course I shall correct them myself. — In the Icones you will see at least 5 different kind of types. I do not know how to match them for the printer. Take for example the 1st. Plate.

Sphagnum macrophyllum. Bernh. 2
Table 1. 1

Plantae caulis etc. 1

Syn: *Sphagnum macrophyllum* 1

References 1

etc. that makes 6 different types. Can not the printer fix the text in comparison with that of the first vol. or shall I underline and how. I do not know of any indicative rule about that.

I have this morning a sad letter from Baron de Buren's son. Perhaps you know that excellent man, good botanist, friend of Boissier, Godet and many others; one of my dearest friends of old with whom I have constantly corresponded. He is dead and now of those who knew him can think.

Columbus 5th March 74

My dear Prof. Gray.

Lindberg writes that he should be glad to work and publish the Venezuela moner, but that he cannot find time for that till Sept^r. - Shall I send package? I do not know Lett. but as one of the best living biologists. He may keep the moner perhaps for a long while. As they are interesting for science I should send them if they were my own, but I can not be answerable for a valuable collection which does not belong to me. I will send him my copy of the Cuban moner, which he desires to see. I have been so harassed by Hayden for his report and suffer so much from my old complaint neuralgia of the optic nerve, caused by over work that I have not yet finished the description of the Tiou. Two weeks of time will put me through and I will have them from beginning of May. I wish to spend two months of the summer or more at Cambridge. I could then correct proofs at that time and spare you the trouble.
Your old friend respectfully. S. Lequereux

Have anyone in about that package which I may send now by
Sh. Smithsonian. I must show you little & full want an authority for
sending away the moner if I be asked to it

Columbus O. March 14th 74

My dear Prof Gray.

Since my last letter to you I have received a copy of the Journal, and read there a notice, just as I wanted and expected to see, of the Cretaceous flore. I will write a short one on Heer's third vol. for the same journal.

Address - Prof. Lindberg Helsingfors
Finland. Russia.

Prof. Carl. Muller Hall, Russia.
Dr. Ernst. Harnpe Blankenburg
Harnz. Braunschweig. or
Germany.

I suppose you will send by The Smithsonian.
Any how I will advise Harnpe Muller
Schimper to whom I have to write that
the vol. has been sent to them by yourself.

I shall be glad to get my copy
of the Suppt and that full copy which
you send me besides, will be kept in

remembrance of Sullivan I thought I
do not want any thing to recall his
memory. He is with me most of the
time.

Sorry to hear that Mr Gray is
unwell as you are also yourself. This
winter has been a very hard one. I
sincerely desire and hope that you
may soon be out of trouble yourself
and your kind lady, to whom please
offer my respectful regards

Yours very truly
A. Desquereux

What is the price of the
Supplement to the Scores? I have
answered to applicants that it (the sup^l)
is for sale by Westerman & Co. But some
ask for the price.

Columbus 17th March '44

My dear Prof. Gray

You did not recollect what I wrote you formerly on those Venezuela moths. Sullivan bought ~~Wentler's~~ collection with numerous specimens, of course. He gave it to Schrader on condition that he (Schrader) should sort the specimens, examine all the species, make good sketches (microscopical) of each species, give to him, (Sullivan) the best set with the sketches and that he would then determine and name them, each species bearing a number. and Schrader could sell the other sets on his own account as a compensation for his trouble and work. Now Schrader says: these moths have not been named and my sets can not be sold; therefore I consider that Sullivan's set with sketches belongs to me as I will send it to Germany for determination of species in order to be able

to dispose of my own sets. In this matter
Mr Sullivan nor myself did agree and the
matter stood thus when I proposed to
Schraden to send the Sullivan's set to
Lindberg to have the money determined
and published and that then Schraden
might get copies of Lindberg's descriptions²
to go with the sets which he intends to
offer for sale. Now then the matter stands.
Sullivan's? set and drawings belong or
should go with his collection. You alone
have to say if I shall send it to Lindberg
who promise to return it or not. If
not, what else^{is} to be done with Schraden?

As soon as I am ready to go to Cambridge,
I intend to pack all the books and collection
of J. which belongs now to your herbarium
and see that all is right according to the
catalogue made by myself and Mr Jos. Sulli-
vant after the death of his brother. There
are some matters in regard to these collections which
should be fixed with you. I went to Sullivan
sometimes he too came to me but we could not meet,
till yesterday. He says he wrote you and sent the books
by our express on even Monday & day.

Cambridge 30th August 74

Dear Prof. Gray.

I was so unwell yesterday that I was unable to walk to the Garden and see you again, which I much regret. I have therefore to say good bye by this note and to thank you for your obliging kindness and good help. - All is fixed with the printer here for the forwarding of the proof. - I have thought much about the possibility of arranging Sullivan's collection and of preparing at the same time the materials for a synopsis of the U. S. moths. This work must be done and certainly if I can find time soon I will undertake it and try my hand at it. Sullivan has prepared it in his herbarium and it would certainly be an easy task to put it in good shape for future American entomologists. - When I have

seen Mr Jos. Tullivant I will write
you and tell you what he thinks of
your arrangements which are certainly
for the best.

Please to remember me very respectfully
to Mr Gray

Your friend very truly
S. Desjardins

Columbus O. Sept. 7th 74

Dear Prof Gray.

That package ^{of mosses} was put with
those of M. James by inadvertance. I have
sent it and I will make the descrip-
tions immediately and send them to you
for publication.

I have rec^d yesterday the 3^d last
vol. of Schimper's splendid work. Paléontologie
végétale. It contains, besides a
supplement for species published since the
beginning of the work, the literature, an
index of specific names, and another of
synonyms, with the rectification of some errors.
This work will greatly help paleontologists.
The last and perhaps the greatest error of Schimper
is his dedication of the work to: Brongniart
Heer, Saporta and Desquereux, whom he calls
les grands promoteurs de la Paléontologie
végétale. The company is too high for me:
but it is too honourable to be declined.
Have you got this work. Will you have some

remarks on it for the Journal; next N^o.
or shall I report on it as I did for
the former vols.?

M^r. Jos. Sullivant is much pleased
with the disposition of his brother's papers.
What you propose for the photograph is very
good and acceptable to M^r. Jos. but I
have good likenesses of our lamented friend
and he will select the best to make
copies upon thin paper. — As with the
am^t. of work which I am forced to put
out of the way before I may go to Cambridge
for Sullivant's collection there will be a good
deal of time, and as perhaps I may not
live till then, I think that the following
plan~~xx~~ would be a good one to present, at
least the mixing of specimens, manus-
cript, notes, sketches etc. That is, to
begin from the first leaf of the first
package, ^(not folio) and mark it N^o. 1 as also all
the specimens, notes etc. in the same leaf
and go through the whole herbarium
marking each leaf with a sequent number.
2. 3. 4. 5. 6. &c. &c.

and also its contents with the same and
thus any kind of confusion would be
prevented. A mere boy could do this easily
under direction of course. What do you
think of it. Is it feasible?

Many thanks for your kind words
about my health. I suffered indeed very
much at Cambridge and reached home
very tired. But I hope to be better & not
quite well soon. I am also grateful for
your kind offices to me.

I expect to receive soon a batch of
proofs of the Iones. As soon as they
come I will read them as carefully
as I am able to do.

Please to offer my respectful regards
to Mrs. Gray. Your friend resp^y
S. Lenz

Note 33 not sent

Columbus O. 15th Sept. 74

My dear Prof. Gray.

You will find herewith pp. 41 to 48 of the suppl. to the Scores. I read the proofs twice as carefully as I can; but I believe that you will still find some corrections to make. Please read the note in pencil p. 42 and erase it after, if all is right. There is also another ? foot of C. 47. - I am indeed sorry that you should have so much trouble in this matter.

You will find too with this James's pamphlet with Sullivan's annotation, and also letters of James and of myself on this matter. If you read them you will see that James ceased to accuse me, only after my offer to submit the matter to your judgement and after he had read a copy of what Sullivan had written upon his Catalogue. The matter has somewhat vexed me because I never intended to wrong James, and to accuse him, as his name was not written or spoken of in any way and

and because he persisted to believe
and to say that I had wronged him.
My regard for him has ever been and is
still the same. - I should therefore
desire that you do not mention to
him the ^{present} communication. Either keep
the pamphlet and letters separate in a
private drawer of yours or rather
return the whole to me to be preserved
in the package of papers to be burned
at my death, just as you like.

I have not got yet those California
mosses. - Where could I get, if only
for a few days the paper of Hooker on
that peculiar Welmitschia, mentioned
by Prof Dana as something analogous
to Conifers of the Carboniferous? You
will much oblige me to let me know
where this description has been published.

My health is much better since I
returned home.

Yours friend very respectfully

L. Desguereux

The printer should send me
a copy of the definitively printed pages for preparing index. I should also have the plates for that same purpose.

Columbus O. 24th Sept. 74.

Dear Prof Gray.

I send back to you galley proofs
p. 49 to 72 sent by the printer. I have not
yet received p. 41-48 revised, which you
mention in your note of the 22^d. I will write you
on that "D. Drummondi" when I have seen
the proof. The remark of Sullivan about D.
Wilsoni is not that it is a new species but
that this species should have been named
D. Wilsoni because Wilson first remarked
that it was not a var. of D. undulatum — at
least if I remember aright.

I will also answer about the mores of Bolander.
To describe them correctly I should have to use the
microscope and though my sight is about restored,
the D. positively forbids me to use the microscope
even for a few hours or one hour a day. Why does
not Barnes describe them? I will, if he does it, review
all his descriptions. And even I will send him
a number of specimens of California mores which
are not yet examined. He could make a good
paper on the whole. Very respy yours
L. S. Dyer

about those Cal^e mores I will
write to Barnes.

first recognized that it was a new species.
I believe that this change was suggested by
Lindberg? perhaps to Tullivant. In my own
opinion, I consider the name as a good one
viz. D. Drummondii who by his discovery
of the moss did more than the descriptive
author. But Muller should have remarked
on Wilson's suggestion. Suppose we correct
the note by writing merely: That the ^(y) Drum-
mondii moss N. 86 was recognized as a
new species by Willson in Hooker's Jour.

Bot., 3, p. 436, (1841.) — Would that
not be satisfactory. If Tullivant had
positively recognized the propriety of making
a species or rather of giving a new name to
this species, he certainly would have done
it. — If you ever go through some part of
Tulliv. herbarium you will find nearly every
specimen ^{and} every species with some kind of
annotation; either as memorandum or as
future reference for publication. If were the
selections of mine may not have been all right

Yours very truly
L. Lesquereux

Columbus O. 25th Sept. 74

Dear Prof Gray.

I return the 3^d proof p. p. 41 to 42.

In regard to that note on Braunii, p. 41
- 42. As the preface of this Suppl. says (or
should say) that the work was ~~pre~~ prepared
from notes and memoranda left by M. Tulliv.
I suppose that it may be thus explained
without the final (-). But if you think
that the remark is necessary, would it not
be better to ^{whole} the paragraph and to add at
the end of it merely (Tulliv. moss).?

P. 46 you do the, after a posteriori but
leave it in the second line after priori. Is it
right so?

About that remark at p. 42. the fact is:
Willson first recognized that Drum^d moss N. 86 was
a new species different from D. undulatum but did
not name it. (Hooker Jour Bot. 3. p. 436. (1841). In
1849 Muller names it D. Drummondii and
1860 Schimper names it D. robustum (Blytt) in hæ-
rium. — Now Tullivant thinks that it would be right
to name it D. Willsonii Sulliv. because Willson

Columbus O. Dec 15th 74

My dear Prof. Gray

Your note of the 10th was received in due time, but I did not answer it immediately, wishing to acknowledge receipt of the copy of the text which has come just now. I am much obliged for the communication. As it is a personal matter, you will allow me to return stamps.

I have no opinion concerning the distribution of the Supplement. As contributors by material and worth, Schroder, Austin, James, Bolander and Wuhu Hall are with both yourself and myself the only ones entitled to a free copy. I think however that one copy should be sent to Muller, Schimper and Lindberg. Both Muller and Schimper are preparing synopses where the Supplement should be quoted. Lindberg has helped Sullivant in some difficult cases of determination; *Orthotricum* and *Funaria* species and is therefore a contributor. The other botanists who have the *Diogenes* will want the Supplement; but they may easily get it as you say by purchase.

I should be very pleased if you would

prepare that notice on Schumpe's ~~vegetable~~
Paleontologie vegetale. Baillière has no direct
correspondant in America; at least Schumpe's
copies which I sold here (6) were sent me by
Westerman. The best is to write to Schumpe
and I will do it Sunday. He will certainly have
a free copy sent to you, I know. Should you
wish however to prepare the notice now, I
may send you a copy not yet reclaimed by
the owner, of the 3^d and last volume. I could
not spare my own copy not even for one week;
but I may borrow one if you can not do with
the 3^d vol. and wish to see it now.

A request still, which your kindness may
excuse. I have an album with photographs of
most of my botanist correspondents. Yours is not
there and I have desired it for a long time. Please
make me a gift of it and also of that of Mr
Gray if possible. Dr Candolle, Father Son, Brissot
Duby etc. etc are there and of Savoy I have Mrs.
Sullivan the second, a good friend and coworker of old,
whom I never see without pleasure and regret.
With kindest and respectful regards to Mr Gray,
your old friend S. Engelman

of Mobile. I suppose also that Sullivant
sent the cones to other European celebrated
botanists: Braun etc. perhaps also to Wright
Chapman, Engelman, Porter, Tuckerman
Ellis Hall. and to some public libraries?

(Columbus 30th Dec^r 44

Dear Prof. Gray.

My copy of the Scones is in black
line-cloth as were the others which I saw at
Mr Sullivant. Mr James' copy is bound in
the same way, I think. And I believe that
yours is the only one which was bound in calf.
But of this I do not know. I prefer to have
my own bound like the first one in black cloth.
You promised me that the printer would send
me a full copy of the text in order that I might
have a spare copy for a friend. Mr S. Sullivant
agreed that this copy of the text should be given
me with the soiled plates used for description.
If you can obtain ^{the text} ~~it~~ from the printer and send it,
I will have it bound ~~in~~ the same as is the first
vol. of the Scones and keep it for me. I shall like
it as well as a new one, as a token of my work for S.
And if I get another copy it will be good for
somebody else. Mr Sullivant wanted me to accept
a calf bound copy. That would not agree with
the 1st vol. and I refused. So it is left of
the nice bound copies and as I believe, yours
only was extra-bound. The best way therefore

seems, to have all the copies bound in the same way as those of the 1st Vol. given out for sale or in black, ^{cloth} even for the copies which are to be presented. But of this matter I know less than yourself. Of course your copy must be bound like the 1st Vol.

Mr. Tullivant wishes me to give you a list of the names to whom his brother would have presented a copy of the Sup^t. if he had been alive. I write herewith the few names of Unitarians who have an interest to the work and were friends of our lamented friend. Perhaps you are better informed on this subject by former communications made for Mr. Tullivant's first vol.

Thanks for your kind wishes. Please accept my own for yours and Mrs. Gray's health and happiness.

Very truly yours

A. T. J. J. J.

That dear Bolander sent me for Christmas a fine ^{bouquet} ~~bouquet~~ from his new garden - Poinsettias indeed. It came per mail and the flowers were tolerably fresh on arrival.

List of names for a
Free Copy of the Sup^t to the Sones.
Prof. Hooker London.

- " Wilson } England. Both are dead, I think.
- " Mitten } at least Wilson is.
- " Carl Muller. Halle Prussia.
- " W. F. Schimper. Strasbourg. Alsatia.
- " Lindberg. Helsingfors. Finland. Russia.
- " P. Hampe. Blankenbourg, Hartz. Germany.
- " Bolander. San Francisco. Cal^{if}.
- " Eaton, New-Haven.

Austin. Cloten.

Ch. F. James Cambridge.

A. Schrader Columbus.

Ch^r H. Gock? Albany This Gentl^{man} the
Marcel of Aiken, Howes etc, sent specimens for
the Musc. Enciclat and got a copy of the
2^d & 3^d of this work. I do not know if Sullivan
would consider them as entitled to a copy of the
Sup^t. Perhaps ~~W. H. H.~~ Olney also. - Friend Bolander
furnished valuable specimens to Mr. Tullivant
even to the last time of his life. He got the
first vol. from me, if I remember well. He must
have the Sup^t. If other names come to mind I
will send them. Prof. Brewer? Dr. Carl Mohr?

1

The third and last volume of W. D. Schimper's *Vegetable Paleontology* (*Traité de Paléontologie Végétale ou la flore du monde primitif etc.*) is out. The outlines of this remarkable work, as exposed in the prospectus of the 1st volume, have been mentioned in this Journal Sept^r 1869. An exception to the general case, the work gives far more than promised. It has one hundred and ten plates (folio) illustrating most of the genera, instead of one hundred ^{which were promised} and both the second and the third volume have each two hundred pages of ~~separate~~ ^{more} matter, more than the first which was already of large size (730 pages). The third volume contains, besides a supplementary addition of the species discovered and described since the beginning of the work in 1869, clear and important remarks on the distribution of the fossil floras in regard to their geological relations and successions, with tables of the characteristic species of each geological group. It has also a full exposition of the literature on vegetable paleontology or an enumeration of all the works published on the subject. The amount of materials presented in Schimper's *Traité* or *Synopsis* is indeed prodigious. The number of species described reach nearly six thousand, distributed in eight hundred and fifty genera which ~~are~~ ^{are} like the species ^{are} clearly ~~and~~ ^{are} characterized ^{and} ~~not~~ ^{in regard to} ~~remarked upon~~ ^{their} limitation, relation etc. As all the American species of fossil plants published before the completion of the work have been carefully recorded

2
in it and as their number is already large, this book
will be as acceptable and useful to Americans as to
European naturalists interested in geological and
paleontological studies.

Perhaps you may find convenient to say something
more. — Schimper dedicates his book to St. Brongniart of Paris,
O. Heer of Zurich, Count de Saporta of Aix, France and
Lev Desquereux of Columbus whom he calls his dearest
friends and the great promoters of Vegetable paleontology.

Schimper worked forcibly at Strasbourg during the
Siege when constantly under the fire of the Prussians and
menaced of the total destruction of his property, his
library, even of the collections which were used for
his descriptions. Some errors of the first volume
have been reproached to him (especially by Dawson).
They have been fully explained in the last volume.
As soon as ready with the fossil plants, Schimper
has begun a second edition of his synopsis *Museum
Europ.* which you well know — I suppose that
you have here more than enough for a kind note
on a scientific work which is certainly of the highest
scientific order, most valuable and also interesting by
the beauty and clearness of style and expression.

Please say nothing of the dedication if you do not
think it honourable to American science. My name is of no matter in it.

Columbus O. 31. Dec^r. 74

Dear Prof Gray.

In one of my former letters I proposed to Prof Dana to send a short notice for the *Saunders* on Schimper's work. As he did not answer to this, I supposed that the offer was not acceptable. It would be far more pleasant to Schimper and more valuable for the book if you should write this notice than if I had to do it, yours being our highest scientific American authority. —

In the first page with this, I have written about what I thought should be said of the *Traité de Paléontologie*. For myself the book is a true encyclopedia and I do not know how I could go along without it. But of course it can not be appreciated here for a time.

I believe however that it will go with *Pictet's* work into all the scientific libraries of this country. I have writ some notice on it and take from the materials herewith any thing convenient to you though little they may be. I have just received your card and I have no time to do better. I have have a copy of your notice for me in order that I may send it to Schimper. Very truly

Wallace should send you a copy of the work for your notice. I will mention it to Schimper. L. Duguesne

Columbus O. Jan'y 28^d. 75.

My dear Prof Gray.

Your kind note and your beautiful card are received. I am indeed grateful for this communication of yours and proud to have such a splendid representation of yours in my album. Sorry that my perhaps indiscreet demand could not be complied with by Mr. Gray. But I still hope for a more friendly decision. As you wish it, I send my own ^{card} statuettes at New York about ten years ago. I can not get any better now. But will send you another as soon as I have courage enough to let beard grow.

If you wish it I will make you a detailed account of the contents of Schimper's north from the beginning to the end and with this, you may easily write notice of the booth. Please let me know. — The Suppl^t of the Musci will sell well, I am certain. Already many have applied to me to know where to get it.

Very truly and thankfully yours
L. Deane

Columbus O. Feby 4th 75

My dear Prof. Gray.

Mr Jos. Sullivant says that he will write to you about the number of copies which they want here. - Indeed I am sorry that you returned sick from Washington. I would say take good care of yourself if you had not Mr Gray to attend to you, better than you can ever do yourself. To your kind lady I am greatly indebted for a communication which though much desired was not expected. I prize the gift very high. The likeness is perfect, and very good, though not quite as good as the original for the good reason that the expression, the soul in the eyes can not be photographed. Please to give Mr Gray my sincere thanks and respects.

Very truly yours

A. L. S. Querey

I wrote to himper one week ago or more. Sent you also lately a letter to Mr. Sullivant and on from her too.

Columbus O. 7th March 75

My dear Prof Gray.

Indeed I do not know what to say about the communication (present) of the Supplement of the Journ. On my first list you remarked, with right, I think, that the suppl^t could be bought by those who had got the 1st vol. in order to get at least the expenses of printing, binding etc. This admitted I do not see that the book has to be presented to others, but to those who have contributed to it by communications. Jarvis, Austin, Elihu Hall, Polander. After these there are still Ransom, Brewer, Deeth, Gortner, Howe, Watson, Mohr and Macon who have sent specimens for examination and are mentioned as discoverers in the vol. But the publication of the discoveries is, I think, compensation enough and if the suppl^t is distributed to all the bryologists it will be a long time till the expenses are paid by the sale. Of foreigners I have named Schimper Lindberg. Carl Muller and Hampe with the great botanist Hooker, Dr Candolle, Her, H. Braun Engelm

etc. etc. With these you are better acquainted than I am. I believe that Mr. S. Sullivan would wish to distribute the book to many of his brother friends and acquaintances. If it is the case, I will give him a list with details or rather address it to you if you take the trouble of forwarding the packages. With all what you have done already in regard to this matter, it would be too much ^{trouble} _^. I will try and see Mr. Sullivan soon. But he is unwell; at least he was, the last time I saw him.

I was very anxious about yourself fearing the results of that bad cold and was happy to see by your kind note that you are yourself again. James wrote me that he had got the Suppl^t but said nothing about yourself also nothing about that Suppl^t which however gives him more credit than to any other contributors.

I got per mail yesterday, the 3^d vol. of the Arctic flora of Heer. Did you not get from him the two first volumes? If you wish to notice the worth in Silliman's I will send you my copy as a present from Heer and you may give me

yours when you get it or if you do not get it I will have another from the author's book seller. For indeed Heer owes me nothing while I owe much to himself and especially to his works. Did you not get a copy of the Cretaceous flora of the Dakota group? The comparison of these two floras of the same age is extremely interesting. Our American Cretaceous is however more recent, at least in regard to its vegetable types, most of all representing dicotyledonous forms. I may write something myself on the subject. Any how both these monographies of Heer and of myself are most valuable as the first important contributions to the Cretaceous flora, ^{really} ~~x~~ totally unknown as yet. Do not refuse to accept my vol. ^{of Heer} if you like it in the least. I can not give the work for a while being deep in the preparation of the Silurian flora and other matters which will take all my time for one year at least.

Very truly yours!

Except a short ^{Latin} diagnosis for each species, the flora is in German. I do not know if you read it easily.

Columbus O. 23 May 75

Dear Prof. Gray

Allow me first to congratulate you on your safe return to Cambridge and especially on the improvement of yours and Mrs Gray's health. May you both be preserved perfectly well for a long time to come. - Then I want to thank you for the communication of your pamphlet (*Compectus of the North American Hydrophyllaceae*) which has interested me much and to thank also Prof Watson for his revision of the *Ceanothus*. It is the more valuable to me that the tertiary has a comparatively large number of species referable to this genus. In offering my grateful acknowledgement of his pamphlet to Prof Watson, please to say to him that I should be much obliged for one or two leaves of *Ceanothus integerrimus*.

And now to Schimper's *Paleontology*. He wrote me on that account especially, 13th March.

I translate what he says:
Speaking about a notice on my book, I
beg you would not in any way trouble yourself
or Dr Gray about it. The editor is a man
despicable (m'p'risable) in whose interest
nothing should be done. I have broken
every kind of relation with him. His
rapacity is such that he certainly would
not give out one copy to have the work
advertised or reported by Prof. Gray.
He has paid me (fr. 3000) three thousand
francs for that enormous work for which
I have worked 5 years and ^{have} spent two thousand
francs to get the literature and the
necessary specimens and when I have asked
him to replace another copy of the 1st vol.
which had been soiled by my too hard and
constant work upon it, he ~~has~~ answered me
that if I wanted one I should have to buy
the whole work. And this thought, I know
by the printer that he has had 100 copies
printed more than it was agreed between us.
Therefore do nothing, absolutely nothing for
helping the business of that miserable Baillien.

From this, of course, you will see that
having bought a copy you need not trouble you
about the book. If you think differently
I will write you a memorandum on the
work. I like it more and more. It is
a whole encyclopedia on vegetable paleo-
zoology. I could do nothing without it.

Saporta is enthusiastic about the
cretaceous flora of ours. He urges me to
push the work forward as far and as fast
as possible. He says rightly: that it is
here that we may expect to find the
origin of the first dicotyledonous forms
and that no problem on the vegetation
is more weighty now than this.
New and remarkable materials have been
obtained since the work is out.

Very truly yours
S. Leger

Columbus O. Oct^{br} 29th 75.

My dear Prof. Gray.

I am to day able to answer your note in regard to the Swens (hepp.¹) and the apparent neglect of reference by the friends of McSullivant. - In his last letter received two weeks ago Schimper says that he found at Tharbourg, in returning from Interlochen where he had spent part of the summer the vol. of the Swens; that he had such ~~be~~ heap of matters to work at after his absence that he could not examine it carefully now, but that he found it to be an admirable complement of an admirable work etc. - Answering him on Sunday next after receipt of your note, I mentioned to him that he should give an account of it in some scientific paper and this very soon. - Last week also I had a letter from Hampe who says that he has received the Suppt. to the Swens and has written his thanks to the

Sullivan's family. - To day I have
a letter from Lindberg who says: (I copy)
"My best thanks for your kind note of
last August and especially for Supp^t to the
James museum which I received a few
weeks ago. It is indeed a most useful
work, one of the best published in Brazil
"gg." After this he answers my letter which
was on account of the determination of those
Venezuela moths of Schröder sent to him
more than one year ago, and which he refuses
to work for want of time - I therefore
conclude that he has not had time yet
to examine carefully the Supp^t and to notice
it somewhere. Or perhaps he has noticed it
but we have not as yet received the reward.

I shall answer his letter on Sunday and
will write to Muller at the same time.
This last Prof^r is also overworked, as we
are all, and in poor health. Be sure
that I shall not let the matter drop.

It appears that the copy of the Supp^t has
been sent by some slow conveyance, perhaps
the Smithsonian, as it has been received so late.

Very truly yours

A. Desjardins

All right about our friend James and the
specimen. He works very well indeed.

Stumbus O. Jan'y. 18th 36.
My dear Prof. Gray.

I return Austin letter which is
on account of the synopsis perfectly satis-
factory. We will therefore go ahead.
Do you object to ^{our} describing the whole
Crypt. flora of the N. A. continent from
Alaska to Mexico? We have now
so many species from the Rocky
Mts that we must forcibly connect
Drummond's mones and taking
Alaska in the area (botanical) we
have to admit the Panadas. These
additions will not make a great deal
of difference in the number of species which
is large indeed. I am working now
the list and distribution. When ready
we will if you please confer about the
mode, especially length of descriptions.
Schimper sends me the 120^{first} pages
of his Syn: 2^d Ed. a splendid work.
He is through but sends all what is
printed. Balme will come soon.

He writes me that he has now ended his bryological career. He can not continue microscopical work. He says that he wishes to dispose of all the surplus of his specimens. He has bought lately Bruch's collection which had been kept by the family for years and he proposes to unite both this collection and his own, and thus to have all the typical specimens of the Bryol. Euroz. and of the Synopsis for distribution. He will divide them in about 10 sets each of about 750 species represented in each set by more than 3000 specimens and offer them for sale at two hundred francs, each. This is very cheap, and I shall try to have some of these sets in this country. You may perhaps help me to that purpose. I will write you more about when the matter is fixed.

Have you in the herbarium *Aristolochia peltata* and if yes could you lend me one leaf for one day.

Faithfully yours

S. Arzquerey

Suppose I should ask shillings for one series of the species described in the new Ed. of the Syn. and not in the first to add to Julliv. herbarium, could you Museum pay it. That would be perhaps 100 species or less. Any how I would ask. -

Columbus O. May 28th 96

Dear Prof. Gray.

Obliged for your kind note. I send to Austin Lindberg's letter. I do not know what he will do. Any how he will answer me. Lindb. is engaged in the preparation of a synopsis or history of the Hepaticae. and I believe that Austin corresponds with him. — Be sure that I shall consider your wishes about Sullivant, herbarium and have done so already for a long time past. If I can find time I shall do as you wish. But as you say I am becoming old and the work to be done still is heaped around like a wall. You know that by yourself. — The death of Brongniart is the loss of a great scientific monument. His life rather belonged to the past than to the present.

Most truly yours

A. Lesqueray.

Did I thank you for that good sketch of *Aristotichia petal.*? It was sufficient to prevent me that the Cretaceous leaf supposed referable to this is not of the genus.

Columbus, 9th. March 76.

Dear Prof Gray

I have to finish this week my annual report for Hayden and the printer is after me. I can not therefore explicitly answer you not just received. If you want more than I say herewith please press. and I will answer in the next next week.

We have already two fine species of *Sequoia* in the Cretaceous (South Kansas, Clay Co.) ^{lat. 39.} One *Sequoia fastigiata* Steud. is an old species described already from Bohemia, Saxony, and lately by Steud. from Greenland. The other is a new species *Sequoia condita* de. of which I have branches cones and leaves. It is in Kansas. Mixed with *Pinus Quercifolia* Steud. of the molten, Saxony flora which is of the quinate leafed section of the *Pseudotsuga* now widely represented in Mexico. To say Japan. Higher than the Cretaceous is found at the top and at the base of the lignitic we have in Wyoming lat 40 three very fine species *Sequoia brevifolia* Steud. represented there by splendid specimens and known from Spitzberg and the Baltic tertiary; *S. longifolia* de,

Columbus 0.12th March 76

Dear Prof Gray

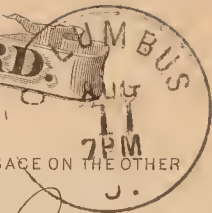
Looking over my notes of the Tertiary
flora of California and Oregon from Prof
Whitney's specimens, I see *Sequoia arvensis*
folia described from Coral-Hollow, San
Joachim Co California. It is there with *Platanus*
dissecta a fine species not yet published, two
species of *Ulmus*, two species of *Laurus*, a
Sindora & *Sabot*. And from Bridge
Creek Oregon I have from specimens of
Prof Whitney also *Sequoia Sanderfordii*
together with *Quercus fuscicornis*, species
of *Ficus*, *Ulmus*, *Cinnamomum* (middle
miocene). San Joachin Co Cal. as represented
by Coral Hollow ^{plants} is upper Miocene.

Yours sincerely
A. S. Sauer

Dear Prof.

Columbus O. August 11th 76

C. Muller writes that he has not got those drawings of the Venezuela mosses. Did you send them by the Smithsonian with that set of mosses? Mr. Schrader here who corresponds with Muller says that if you will send him the set with the drawings he will forward it immediately. Perhaps that would be the best way as the Smithsonian is very slow. Please let me know. - How many species do you know from Cuba? Where could I see some good enumeration or description of the species of the genus? - Going East tomorrow night. Don't know if I shall reach Cambridge. Would be most pleased to see you. Fine letter from Bolander just received. Ever yours S. S. Squire



WRITE THE ADDRESS ON THIS SIDE—THE MESSAGE ON THE OTHER

Prof Aja Gray
Cambridge

Mars

see again - a number of plants in your
herbarium before its publication. The
work is large. 65 plates, representing
species of four different stages of the
tertiary. - The list of our N.A.
mona is nearly ready. This work also
is larger than I thought. I will give
all my time to the preparation of the
description as soon as that tertiary flora
for Hayden is done

Yours friend sincerely & truly
S. Desguereux

With sincere wishes for a
happy year for yourself and
Mrs. Gray, to whom please offer
my most respectful regards

Columbus O. 8th Juny 77.

My dear Prof Gray.

I am now closing the remarks on
the flora of the Chalk Bluff, Cal^a. for
Prof Whitney. This small flor., 50 species
only will interest you very much, as
it represents many or at least a number
of the Genera which out of the Cal^a. flore
are now predominant in that of the Eastern
slope. With one Sabal, it has Fraxus. fer-
ruginea; ; species of Quercus of the Virens
type of that of Q. castanea and Q. lyrata
~~Coastal~~ Liquidambar, Ulmus, two species,
Ficus., Magnolia, an Acer of the spicata
section, Rhus typhina. & R. metopium
with four species of Juglans. etc. This
remarkably confirms your remarks in
the memoir of the Japan flore p. 1449.

In order to supply to the desire^{even} of

materials for comparison, I have sent
the plates to Saporta with my determination
and therefore, these are more reliable &
trustworthy though most of all their deter-
minations have been confirmed by him.
I still want to compare a few species
with living plants. Could you not lend
me for one day only the following ones:
X Aliquidambar species of China. I have
one with small trilobed leaves denticulate
on the border which says Saporta is related
to a Sapan species (which?) I rather supposed
it to be a form of Acer; but I have found
also a five-lobed large leaf with show more
X analogy to Aliquidambar. Schinus molle
horridus; Aralia Whitneyi resemble greatly
A. papirifera by the size and location
of its leaves but the leaves are not dentate.
It is the same in A. horridus; but I would

like to compare, also Q. crassifolia, Hum-
boldtii etc. of that Mexican type. I have
a few detached leaves which you gave me
but not named. Benthamia japonica,
X Cornus semilis, Salix capreoides of Ander-
son. If you send the whole plants please
do it per express at my cost. The
specimens between boards will be
safe. If not a few leaves, one of each
species might be sufficient.

Should you like to read the conclusion
of the flora viz. the whole, about 70
pages ^{210 plates} mss., I am sure Prof Whitney would
not object to the communication. And
I should be extremely pleased to have
remarks and corrections from you.

The fossil flora of the lignite of
the North mountain is far advanced to
its conclusion. But I must if possible

Columbus 4th Jan'y 78.

Dear Prof. Gray:

Many thanks for your kind note and the good wishes of New Year which I reciprocate most heartily. May you have for the future long years of plenty for yourself and kind lady and besides health and every other good thing of this world and for us the result of a work which has done so much for the advancement of our beloved science.

If I was to write you as often as I should wish, as often as I come to you in thoughts and grateful remembrance you would be bored by my letters, at least every week if not every day. But I have the experience of the workers and well know how little time you have even for reading friendly letters still less for answering.

The Eocene flora is soon ready. After your return from the Rocky mountains and a reminder of Prof. Whitney that my conclusions should perhaps be more specifically treated I took the subject again and though these conclusions are not modified in any way, they are a little more definitely explained. With the tertiary flora also finished and retarded only by the printer's work.

for Congress the ^{please} flora would well give a
good account of the distribution of our vegetation.
at least I think so. If you read at least
the generalities of these two memoirs, please
to judge them leniently. You know by what
disadvantages my work is encumbered.

Allow me to offer to friend Mrs Gray my
highest regards and respectful compliments
Your old friend

A. Engstrom

I forget the eventual answer to make to
you Carl. I saw Schroder sometime ago and
he asked me if the Venezuela money had been
sent to Muller. I did not know. I see him
about every week; as soon as I have an
answer to your question I will let you know.
My sight is becoming very poor and the eyes
trouble me much

The Miocene Flora of barlow is very closely allied to that of Oregon, Alaska, Greenland, the Baltic, Oeningen etc. but as yet its types are not clearly defined in other groups of the Lignitic of the Rocky mountains while a number of them are in the Pliocene of California. We know as yet too little of our Fossil plants and the future will show a great deal more for the field is immensely rich.

Should you want any kind of information on any part of Hayden's and Whitney's plants, I would answer immediately though I cannot write much myself now.

You advised our friend James for the best. If he can fix the determination of the few doubtful species of our moss, the synopsis can be made here easily.

Sincerely your friend
L. Lesqueray

Columbus. O. March. 9. 1878.

Dear Prof.

You have by this time received a copy of the Pliocene Flora of the auriferous deposits of California. Perhaps you may like to ^{have} in the Journal a short review of this and of the tertiary Flora and thus I take the liberty to make a few remarks which may be used or ^{not} just as you like.

These three Floras may be considered as a same work and explain one another. The first, ^{the brilliant} exposes the origin of some of the predominant types of our present vegetation. In the second which especially expose the tertiary character of the Flora, ~~this~~ types becomes more and more distinct and numerous. Their relation to those of the Eocene and

of the Miocene of Europe are of course especially considered as proof of the tertiary character of the formation. The third as Pliocene especially considers the relation of types to those of our time. If you can read the conclusion of this last part, you will easily understand the whole.

The plan of the work was not fixed from the beginning for I had to determine the materials by series as fast as they were sent to me. I then considered as extraordinary the peculiar development of the dicotyledonous in the Cretaceous which I ~~considered~~ ^{admitted} as a proof of disruption from those of the Jurassic. This idea was confirmed by the preponderance of palms in the lower Eocene while there is none in the Cretaceous. But I found later, types of Cretaceous

Conifers and dicotyledonous in the lower tertiary strata of Point of Rocks and after a while I was forced to abandon preconceived ideas and to do as DeCandolle did for his Geography, to go along recording facts and leaving them speak for themselves.

Besides some very remarkable types described from the different stages or groups of plants, that of *Delaginella* in the lower Eocene for example, there are some ^{interesting} ~~remarkable~~ cases of distribution of species. For example Point of Rocks is separated from the upper strata of the Dakota group and not a single species has been found identical between those two formations, while the Flora of Point of Rocks is about the same as that of Black Butte which is 3000 feet higher in the measures

Columbus O April 28th 78

My dear Prof Gray

Being a little better to day, I am able to look a little more closely about that description of Ficus spectabilis and to write a few words completing my letter of yesterday. The mss. as you will see on the page herewith (which please return) has "broadly ovate narrowed upwards to an acumen". This in looking to fig. 4 of Pl 33 is not correct for the leaf is rather lanceolate-acuminate, and on this I changed upon the galley proofs. ~~into~~ the sentence as it is rightly printed in the flora. Therefore the error is of mine not on the printer. I am however not certain that for new leaves as are our so called species this description is wrong and I may even have written for three leaves, one ovate, the other lanceolate the third nearly linear "leaves ovate, lanceolate, linear, in reference of course to the leaves figured as are those of this Ficus. Two descriptions

made from whole plants as in the
botany of living species, this would be
wrong; but I still question if it is to
for leaves separated and whose general
character can not be ascertained from
the specimens. The description of this
species in Annual Report 1872 p. 379. leaves
& leaves - broadly ovate-lanceolate contracted
to a short point etc. which is not right for
the contraction is not distinctly marked and
some leaves are narrowed to ~~an~~ acuminate rather
long than short.

But this will not atone for deficiencies
in the description, no more than the bluffed
tips of my corrections of the proof in running
around in Ferns and I am sorry to say
that Dr. Hooker will find many other things
to complain of in this work. Please to say
to him to put all the deficiency on my account
and not on that poor vegetable paleontology
which is the history of the vegetation of the world
is a series of hieroglyphic figures whose decipher-
ing is merely at the beginning. The first attempt
to the reading is very unsatisfactory. Others

will do better hereafter.

I must go to New York to see my doctor
and perhaps will stay out a few weeks to
get rid of the intermitted fever my worst
enemy. I should like to go to Cambridge
and see you to have a talk about mosses,
Jamaica, Austin and other matters. But I
do not know if I shall be able to go
so far.

Give my highest respect to Dr.
Hooker and my thanks for any of his
remarks.

Yours old friend
A. S. Senguer

Columbus O Nov^r 28th 79

Dear Prof Gray.

I am now closing the descriptions of the species of the Coal flora by the examination of the Cordaites and the fruits. We have on these plants splendid materials which may perhaps show their relation with some more evidence than it has been done before, even by Grand'Eury. But I am greatly in need of materials for comparison. Have you perhaps or heard cones or fruit of Guaidea and Garnia. I have a very fine cone of Lycus and the nuts (separated) of one or two species but none of Garnia. Where could I find good illustration of these two families. Of the Conifers I should like to see a fruit (ripe) of Podocarpus. Where to find a good description of Forreya Californica. Bolander sent me long time ago in a bottle a large fruit labelled Forreya Californica, pericarp fleshy, the nut large than an almond in distinctly vertate lengthwise. Is that Forreya Californica? Please excuse

me for troubling you with questions.

I have now two species of large fruits,
one *Cordaiarpus* of *Cordaites Costata*,
another *C. Mansfieldi* same as that
fig. 21. Pl. ~~LXXXV~~ ^{of the Atlas of the Coal flora.} only slightly smaller, both
attached to a branch or to their supports.
That of the plate is over turned, the notch
at the top is the point of attachment. I
have also the flowers of two or three species
attached to stem bearing leaves and many
other new and interesting materials useful
to this family. —

My health is very indifferent now.
I am overworked perhaps. But the Coal
flora is soon ready and I shall then come
to mosses and give my whole time to the
preparation of the Manual.

Recall me please to the kind remembrance
of Mr. Gray. I hope that you are both well.
Respectfully your friend

L. Ingheney

I can send per express any materials
you may lend and send at my ~~expense~~ expenses.

Columbus O May 10th 80.

Dear Prof Gray.

Schimper's son, already a good botanist, he has especially studied physiology and anatomy (vegetable) and says that if he had some prospect to find here a position as professor, he should like to come and settle in America in a year or so when his father's business is arranged. Don't you think that a good physiologist would be a valuable acquisition for botany here.

Should I not encourage him in his purpose? He is not without ^{means} and could live some times on his own small fortune. Please tell me what you think of the matter.

From informations received long time ago of the young man (24 years ^{old}) he is very clever, modest, ardent worker, a true student.

Now my health is much better, indeed it seems that I have got a new lease of life. I am now working hard closing the text of the coal flora, clearing field around me in order to be able to take to the hygienic of

the more without hindrances of any kind.
Once at that work, I shall not have it untill
done, if my health continue good. I am
with admirably well in preparing the
ways by reviewing all uncertain species
and procuring documents.

Please to offer kindest respects to Mrs
Gray who, I hope is like you in good
health.

Always and respectfully your
sincere friend

J. Sequeroy

Columbus 06th Feby 82.

Dear Grief Gray.

Your kind & ad letter was duly received, about the same day, I suppose, as my own reached you. I have partly answered what you ask me about money. The work is too far advanced to ^{be} abandoned and as the more difficult species, those of Audin and the specimens of Macdon have been discovered long time ago, from materials (sketches) prepared by James, the way is clear enough. If I had all my time free I should be soon ready. But I must live and as my only income is that afforded to me by paleontology, I have always a hand some worth in preparation and can not abandon them of course since I am paid for them.

If ever you have a chance to do it, when Mr James' family is quieted after the sorrow of the loss, you would oblige me to ask Mr James how if I can keep for a while longer the two bottles I have belonging to his father. They are Bryozoa Britannica of Willson and vol 12. Journal of Linnaean Society of London.

There are still here two small pamphlets of
Jaeger which do not belong to Mr James but
which were lent to him. I can return them
at any time. Also it would be important
to find my manuscript of the Sphagnaceae
which I sent to Mr James and of which
I do not have any copy. It is to be merely
reviewed like all the other of my descriptions
and it is myself who have to do the kind
of work as Mr James could not attend to it
except for the change of expressions or things.
I intend to shorten the descriptions very much.
As I told you in my former note, the work
must be published in both names and as the
property of both James and myself. - Perhaps
you may learn what will become of Mr
James' collection of mosses. It is valuable and
should go to Harvard and enter into that of
Sullivan. Of course I shall greatly need the
enumeration of some notes and specimens of
Sullivan. But I have 5 volumes of my notes
written when we were working both together
and most of the important remarks of
Sullivan are quoted there.

I know that you are over-crowded by work
and can not find time to answer my letter.
If however you can help me by advice or
by any suggestion you will, I am sure, find
a moment to write to me about.

It is a pity that Austin would or could
not keep his engagements and work the
Hepaticae. This part is essential with the
Mosses. I see in the Botanical Gazette a
list of Hepaticae (North Am.) by Lucian
M. Underwood of Bloomington. Perhaps he
would or could undertake description of them.
Do you know him. He wrote me some-
times. - Though I gave to Austin all I had on
Hepaticae, Booth and specimens, I have still
some essential works, Synopsis of N. Am. etc.
and could perhaps review the matter.

I am truly happy to know that Mrs
Grays' health like yours also is quite satisfactory.
May this blessing be continued to both for
a long time. My health is also good enough
now. But the infirmities of old age can not
be spared. I am scarcely two years younger than
James was. - Very respectfully your old friend
J. Lesquereux

Columbus O. 15th April 89.

Dear Prof. Gray.

Could you, without too much trouble find among your pamphlets Lindbergs *Manipulus Musc. secundus* 1874. He sent me a copy through Sullivan, but it has been probably lost for I never received it. He wrote to me about and said he was sorry he could not send another, but that yourself or Sullivan could lend it to me. James has seen it at you. It has a remark on Thuridium which I greatly want to see. If you can not find it without taking too much of your time, please let me know and I will write Lindberg for that note.

I hope that this will find you in good health

Yours friend respectfully

L. Serquere

What will become of James collection and books. He had offered to lend me some of them. I may need the sphagnaceae of Chautwait but not now.

Columbus O Sept^r 9^o 82.

Dear Prof Gray.

I hope that you have enjoyed a good time of rest and are now returned fortified and refreshed to your work. May this letter find you all well with Mr Gray to whom I send my respectful regards.

I have finished the descriptive part of the Synopsis of the American mosses, about two weeks ago and already prepared a short introduction. When I have made a kind of key which would be really more useful than a. conspectus, I will begin the copy with abbreviations and corrections of the manuscript and hope that the work will then go fast and be easy. For species of Hypnaceae which were uncertain and had not been examined by James, I have found ~~a~~ valuable assistance in Europe. Cap. Renault a bryologist of France who has for years studied the most difficult part of the Hypnum, the subgenus Harpidium, has already reviewed most of our specimens which were still undetermined or not satisfactorily analyzed and is still continuing the work. I believe therefore that little shall be left

unnoticed.

I should now like very much if Mrs. James could carefully look over her husband's materials and find my manuscript description of the *Sphagna*. At least, I want to know if it is definitely lost, for then I will at once make the work again. If I do not mistake, friend James desired to have it in order to show you if the descriptions were not too long. I shall certainly abridge a great deal. But it is very difficult to make short descriptions giving a diagnosis sufficiently clear and precise, as well you know. I think that it is more difficult for mosses than for any other kind of vegetables.

As we have the plates for the genera, would it not be advisable to add to the mosses the descriptions of the *Hepaticae* as made by Hiller and others. That is not much perhaps, but that is some thing and certainly a great deal of help for beginners. Perhaps the species described as new by Austin could be added. But he has mixed matters very much as well in the *Hepaticae* as in the mosses and it would be difficult to find the right place at least for his new genera.

I know that you are always overcrowded with work and for this reason I have not written to you before and will not now, although I have or different matters concerning the descriptions of the mosses and shall be especially embarrassed for the typography. As I have done it, it may be too short, for I generally give the first name or at least the name now generally admitted by authors really the first author's name and also the one by whom the species has been illustrated like the *Oryza* *Eu.* and the *Sonchus*. — How much I need Silliman's herbarium for his notes and James's too for his specimens which I could send to Europe for revision. Austin's *Musc. Apal.* would be very acceptable and perhaps Mrs. James would send it to me as I could take from it parcels ^{or bits of specimens} of those new species which he has described and which still puzzle me some. What will become of James's mosses? I hope to get soon news of Mrs. James and will ask her for what I may need.

Your old friend respectfully
S. D. W. Carey

Columbus O 18th Sept 89.

examined by James and some which have not
been satisfactorily determined from his sketches.

Burchell of France has revised already our species
of Harpidium. He is now after the Hypnoidae.
I give him specimens, he promises to go on
with me to the end. After all there are not many
changes to be made. We know most of the differ-
ence of opinions and the value of the synonymy
admitted by authors by Sullivants notes and my
own. The most difficult species are those established
by Austin who by a spirit of contradiction de-
scribed a new species, poor variety of the most
common ^{genera} ~~species~~ like Cratodon and considered
as mere varieties species fixed by the authorship
of Schimper, Sullivant, Muller & others.

I have just received a note of Mr
James who has found the mass of *Phacium*
and has sent it to me (not yet received).
The interest in the synopsis of mosses is great
I get very numerous letters about it.

Sincerely yours

L. Lesqueray

No charge for the volumin

I have also by this morning mail a letter from Bolander who is
returning from Europe and whom I have not seen nor heard of him for
years. He does not give any address. If you see him tell him to stop here and
see me.

Dear Prof Gray.

We tried with my son to get some of those
roots of *Neolumbium* according to your direction.
But we could get nothing good. The stolons are
very long $\frac{1}{2}$ to 1 inch in diameter, rather small
bearing here and there bundles of rootlets and
stems of the year. As I could not get anyone
there to dig in water as long a trench as neces-
sary to find buds for next year, I took merely
the part I send you with ^{the} roots. The water
at the lowest place is about 1 foot deep and
of course my son who is a business man
and knows nothing about digging under water
would do no' much. There is no people at
the Reservoir who could understand and
undertake the job. There is only a tavern
for people who go there for fishing and hun-
ting and as there is Sunday during the summer,
an excursion train going there only at a cheap
price, I have been there sometimes for a day
of relaxation. Yesterday was the last Sunday
these excursion trains were running. Should
you badly need these roots and wish me
to try again I would have to take a man

there from Columbus and the cost with expense of transportation could not be less than \$10.~ This with the uncertainty of success and the loss of my time would I think be too much. The landlord of the tavern said to my son that he could not succeed to reproduce the Nelumbium from stolons or roots but that in throwing ~~the~~ seeds in spring, just after the ice was disappeared, he had formed around ~~the~~ and near the borders small patches ^{of Helophium} where he could every year get flowers for people who come after them. The nuts top of the bottle and still green, are already ripe. It is in that degree of ripeness that they are food for the Market Plats. which abound there. As most of the seeds are still unripe, I can obtain some more later. At least the man promised to gather them for me.

I am often somewhat embarrassed for details of little importance in the preparation of the synopsis of the American mones. This is especially the case for synonymy. Following Schimper's method, I write with the specific name the name of the first author who has

examined and determined the species and in the synonymy the name of the last or admitted author of the genus. Thus:

Micromitrium synovicum, James.
and after the description:

Sphenomerum synovicum James. Trans. Am. Soc. etc.

Micromitrium synovicum Austrian. Musc. Sp. etc.

I think this is right. - About synonymy of different authors merely indicating changes of names, I do not indicate many of these different changes generally mentioning the names and authors when the species is illustrated. Perhaps it would be best to send you a few pages of my mss. and you would if time permitted to you have the kindness to suggest any changes to be made. I have of course a systematic conspectus of the character and distribution of the genera, instead of, or serving as a Key. It is ready but not copied yet as I fix it definitely only after the description of each genus and their species, in order to make it clear and to say nothing useless. Well I am doing my best and find helpful friends in Europe for the most difficult parts which have not been

Columbus, 0 Sep. 28th 89.

Dear Prof Gray.

Excuse me for troubling you once more about that synonymy. I have of course that leaf which you had printed for direction to Friend James. But just in that, we do not agree. I give with the name of the species the name of the author who described first the species. Thus you write:

Micrometriscus synovicus Austin and
after description the synonyms and references are
M. synovicus Austin Mus. Acad. etc.

Ephemeriscus synovicus James etc.

Nanometriscus synovicus Lendb.

While I should like to write and have written

Micrometriscus synovicus James,
and after description etc. for synonymy I
refer.

Ephemeriscus synovicus, James etc.

Micrometriscus synovicus, Austin.

Nanometriscus - Lendb.

Which is right? In order to spare your time
please erase the one which is wrong. But
indeed, I do not see any reason to change
my mode of quotation. - Also I don't

mention any synonymy of genera as most of the important ~~specimens~~ ~~of a~~ name of genera are used as synonyms with species.

The nomenclature of genera, is, especially for the mones a matter of fact and one who likes to have his name published may put that subdivision of genera at infinity as well as the substitution of new names. I therefore do not see why one should give the first place to the author of a genus. For example, the genus *Microstridium*, is separated from *Ephemerum* on very little ground; only the capsule, ~~is~~ very small in the first, slightly longer and campanulate in the second. Schimper, ~~and~~ Bruch, Muller, Sullivan and all the others did not make a genus to separate *Ephemerum tenerum*.

That was reserved for Austin and after him for Sanderby who put his *Nanostridium* instead.

If there is not any positive rule forcing the nomenclature as it is made in your paper instead of that I have used, do not trouble you to answer to your old friend
Sincerely

That I find good E. Hall is gone. He was one of the best collectors of Botanical specimens, as well you know.

Columbus O March 3^d 83

Dear Prof Gray.

At least, I have finished the ms^t of the Synopsis of the North American mosses. That is a good title, I think, the book describing species of mosses whose habitat ranges from Florida and South California as far North as the Arctic zone, even Greenland. — I have now to review my table of classification or key which is about like that of Prof Watson in Bot. of the California Survey, but of course of far wider extension, and then I must read again the whole to uniformly arrange and number the pages, the divisions, the species etc. There will be also some change to be made in the references or the synonymy and I have to wait news from Lindberg and Muller to whom I have written on a few of their species which are as yet unpublished. — I have in the ms^t separated as genera all the subdivisions of the Genus Hypnum, established by Schimper and Sullivant. Schimper admit most of them as Genera; Sullivant has admitted them also in

his Supplement to the Icones. Perhaps it would be best to consider them as subgenera, as Sullivant has done in Mones of the U. States, and Prof Watson also in his Bot. of California. The present tendency is to subdivide indefinitely, as you have seen in Brachyotites Lindberg and others. But as all the north American mones are made according to Schimper's classification, I follow Schimper as did also Sullivant. But on this I am still undecided, either to make or admit the subdivisions of Hypnum as genera or groups?

What do you say about Bryojamesia. Is not such an arrangement of name somewhat exceptional and subject to criticism? I have made a new genus or subdivision of Hypnum. (Claspodium) for species intermediate to Thuidium and Luzurius. H. Whippleanum, H. crispifolium, H. laxifolium. H. ramulosum & H. Whippleanum. Without mentioning the position of the capsule, abruptly horizontal as if the top of the pedicel was broken, the characters of this group which is entirely West-American are quite distinct. I could thus take the name of Bryojamesia instead of Claspodium if I admit

the subdivisions of Hypnum as genera. But as naming a group, that would not be appropriate and even as genus, I do not much like it.

I do not want to trouble you about details of my work. I wish only you could look over a few pages in order to say if the description are not perhaps too long; if the arrangement is convenient. If then you will allow me to send you a few pages as sample, please let me know what part you would like to see.

I should prefer to send you these pages soon instead of waiting until the whole is ready, as it would be inconvenient to begin again ^{the} review and correction of a work which I have already written three times (including the original catalogue) and read at least five times through. That work is indeed one of devotion to science; for I have nothing to expect from it and it has taken at least three years of my time. But James has had no more than mine. Very truly and respectfully yours
S. J. Vasey.

style, but she says nothing in her letter about that
purpose. ^{of purchasing my right of property} I have mentioned that in my answer
addressed to Philadelphia. - I have made a good
general division as James'ello. - Is not the title
Synopsis of the North American mosses better
than Manual?

I should have still more to write about
that work which has taken so much of my time
for not material advantage whatever, which has been
made from my love for the mosses, from friendship
for Sullivant and James and from devotion to American
Science. All this you know and for what I should
need of your advice on this subject I know that you
will be kind enough to give it to me when needed.

With the highest respect and sincere affection

your old friend L. Sequenzey

I forgot to mention the plates to be made. Very
few genera need exemplification. But we may have a
VIth plate with subdivisions (genera) of Hypnum. Little
pl. VII of Schn. ^{synops.} Most of the drawings may be copied from
the last plate; a few others may be made by my
draughtsman. If you can, please direct me what to
do for that and when I shall apply for engraving.

Cumtibus April 9th 83.

My dear Prof Gray.

After writing you that I was ready with
the manuscript of the synopsis, I was confined for
some days by a spell of intermittent fever and
forced to quit work. When a little better, I thought
that the best I had to do was to review once again
the ms. and to send you the whole after definitive
correction. For you could not judge the work from the
examination of a few pages only. I wished to send
the ms. through Mr. James to spare you some trouble,
but Saturday I was advised by a letter that Mr. James
and whole family leave Cambridge day after to-
morrow for Philadelphia and start for Europe 1st of
May. Hence I must direct the ms. to you, what
I do per Express to day, paying charges of course.
You must excuse me for the trouble forced upon you
by that matter. I know how hard you are pressed
by work and I should much like to spare you
the necessary trouble. This will give you.

I have in the ms. followed A. Watson's method
as closely as possible. For this and in order to
conform the nomenclature of the synopsis to what
you consider the best way of quoting authorship

and synonymy, I have as you will see erased
all my first arrangement. Indeed, no much
has been changed in the descriptions, for I have
made them as carefully and as clear as possible.
Perhaps some of them, especially the remarks are
a little long; but I may, if you find ~~the~~ modifi-
cation advisable, correct that in reading the galley.
And as nearly all the work has been copied
from the original ms. the printer will have
no trouble. — I send with this the proofs which
Mr Watson had the kindness to lend me and
also the 3 pages which you had printed in order
to show the arrangement and the different sorts
of type for printing. I find that of Mr Watson
very satisfactory, viz. largest type for titles, subdi-
visions; "Spurious names; Suborder, Tribes; then
for the Genera ^(with the middle) ^{etc.} the species; then the common type
for descriptions and quotations of authors; the
description of groups or sections in Italic, and
the habitats in small type. All just as in Mr
Watson, which indeed is an excellent method
and looks beautifully. But of this, you know
much better than myself. If you approve of
it, and if Mr Watson does not object,

the printer will find the best directions in the
examination of a few pages of the proofs returned
with the ms.

Should it not be advisable to preface the work by
a few words of historical remarks about like the
two pages which are top of the ms. If they are
not advisable, please throw them aside.

If you see Mr. Cassino, please to say to him that
I will correct the galley first and then revise also very
carefully and without retard. He should send me always
two copies of the proof and of the galley. I wish also
to get a copy of the agreement which has been made
between the publisher and Mr James and myself. I
have signed it, but I do not know that Mr James
has got a copy of it. Have the kindness to claim
one before delivering the manuscript.

As an inducement to continue and finish the work
on American moss, you write me that Mr James
would be disposed to purchase my right of property
in the future sale of the work. Mr James says no-
thing whatever about that in her letter. She has
paid for the copy of the ms.; this was right, for
it was agreed with James that he should make
a clean copy of my ms. and correct my errors.

Columbus 0 May 2/83

Dear Prof Gray.

On Monday, I have received from Mr. James the documents I had to sign for the sale of my part in the property of the Synopsis of the American Mosier and the amount, \$4000 paid for it. It is to you that I owe the favourable settlement of this matter, which has been most acceptable to me, for I never supposed that I should get any remuneration in money for this long and tedious work. For this reason, I give you my heartiest thanks, for in that matter, as in many others you have given me proof of your kind friendship. What you wrote to Mr. James is true. The offer made to me is liberal. But I have had and shall have still a great deal of trouble and work until the book is published. It is now in great demand and I have received many letters of inquiry about the time and where it will be published.

I am somewhat uncertain about the mode of preparing the supplementary plate; that is, about the kind of engraving desirable. Could the plate be engraved after the new cheaper

process of photogeaphy? The young man
who works for me draw as well for photo-
-engraving than for steel engraving or lithogra-
-phy. But there is always a marked difference
in hints and this perhaps should be avoided.
I know how difficult it is for you to write and
how little time you have to spare. But perhaps
Prof. Watson would have the kindness to
write upon a Postal card: which of the two
kinds of engraving you prefer. As Schimper
has copied most of the figures in Sullivant's Mon.
I will take some of his own for the vith plate and
have original drawings for a few American genera
of the Hypericeae. Of course I will submit the
plate to your inspection. I wish to make the
book just as good as possible and will spare nothing
for that.

Could the Botanical garden or the Herbarium
spare for me a good leaf fully developed of
Xanthorrhiza?

I have as yet heard nothing of M. Cassino.
But I suppose that when once started the printing
of the Syn: will go fast.

With my best thanks, gratefully and sincerely
yours J. Lesquereux

Columbus 012th May 83.

My dear Prof Gray.

It would be very difficult for me to leave Columbus now, even for one week. I have with me a granddaughter who takes care of me and attends to high school. Her parents do not live in town and I can not leave her alone nor send her home. Besides, I am very infirm now by the influence of a long winter, withness etc. I could not walk a distance of a fourth of a mile nor can I work more than three hours consecutively without rest. As the weather is still cold, I fear the return of some lung affection like the one from which I am only recovering. I purposed to leave Columbus in June after the close of the schools here and go East to rest and perhaps receive some strength and as I have friends and relations at Boston and around, ^{I wished} to spend some time at Cambridge. ~~From~~ I greatly need ~~to confer with you~~ ^{to confer with you}. The type of the page you send me is very fine, clear, easily read; perhaps as you say a little broad. But I am not a competent judge. Only it pleases me much I shall begin at once the Introduction

and make it fuller, as you say. My introduction, as it is now, is much longer and more detailed (explicative) than that of Sullivant and also that of Mr. Watson. I much desired to have it longer; but I had always in mind the necessity of short descriptions & short explanation. — I will also make a full description of the Bryaceae and Sphagnaceae etc. That of the Sphagnaceae, is I think already made. In my last letter, I mentioned merely the word description after Sphagnaceae for abbreviation or shortening my letter. I will send you Monday a new Introduction and the description of the orders etc. and then if you still find that it would be best for me to go to Cambridge, I will see what can be possibly done — I should certainly be happy to see you and to consult with you and am sincerely obliged and grateful for your kind invitation. But you would be tired to death of me in a few hours. Consider what it is to converse with a man as I am now. For I am really deaf and dumb. You can not understand my language and

my memory has become a blank. You would be discouraged in no time. But I say again: If you think I had better come now, I shall try and come if possible.

Very sincerely yours

I. S. Sargent

Columbus O May 14th 83.

My dear Prof Gray.

Your good so very kind letter of the 11th which I have just received impresses me with a deep sense of gratitude and at the same time of regret for the trouble imposed upon you by the publication of the manuscript. I wished to spare you all that. And if James had been lived, he would have had to make the corrections under your guidance and all would have been far more convenient to you. I fully approve your remarks and your critical adviser. But I am under great difficulties in working moves which I have not studied for years; and unknown to the Bryozoists by any recent work, I have to follow the new opinions, really after against my scientific conscience. With very few modifications, Watson's synopsis is quite as good as that of Schimper and much more simple and easy to understand. But I see that it will not be possible for me to arrange matters here without conferring with you on many points. Discouraged as I have often been with this work and am still, I think it must be put through it

The end as best as I can. - I wrote you
on the 12th about your proposition of going to
Cambridge. Yesterday I have seen my daughter
and proposed to her to come to my house
and can for her daughter's school duties etc
untill the time of the vacation. The schools
are closed on the 21st June. She is willing
to come here on the 14th. And therefore on
that day or in one month I may leave
for Cambridge, and stay there not merely
for the beginning of the printing of the Musc.
but for correcting proofs untill matters are
in such a state that you can have no trouble
hereafter. I may easily find a boarding either
at Chelsea with a grand daughter of mine or
somewhere else near the sea which perhaps would
involuntarily me and go to you for advice where
ver there is some trouble in the preparation of
the musc. - Do you think that could be
done? I know you should have a terrible time
with me (deaf and nearly dumb) but at least
we could arrange minor matters more easily.

I send you herewith that introduction or
rather description of the Byaceae. Of course

I can not repeat in it the terms and description
of the divers organs already given in the introduc-
tion or description of the Musc. M. Watson
descriptive what you proposed to copy is for the
Musc.; not for one of the orders. I will ~~and~~
may easily add more details to the one I have
made preceding the conspectus. This also I
will review and make entirely new according
to what you advise; or perhaps make a short
key. But then new materials cannot be ready
before two or three weeks and I do not know what
you have arranged with M. Canino for the time.
I leave only to let me know by a few words if
the arrangement which I propose is satisfactory
to yourself. ~~And~~ I forgot in my former
letter to thank you for that leaf of Zanthoxa
It is quite sufficient.

I am glad to know that you are still
so young 73 and have many years in perspec-
tive for the advancement of the American
Botany. I am now 77! and used up
entirely. You can understand how unable I
am to make ~~the~~ the most necessary work
I thank you again and remain your grateful
friend & D. S. Quercus

Columbus O. 21st May 83.

My dear Prof Gray

Yes, as you say, it is hard for me to begin again a work which has given me already too much trouble. But I do not know how I could complain when I see the trouble you take upon yourself for examining, correcting ~~my~~ manuscript and trying by repeated directions to make our Manual more acceptable to students, and this too when you have yourself on hand a very difficult work more important indeed than our mosses and which demands all your time. I can but thank you again and try to follow your directions.

You want me to make a key like the one of Sullivant. Would not a Conspectus more abridged than the one I have made, be sufficient and more easy to follow by students than a key. For ⁱⁿ a short key like that of Sullivant, which merely considers the capsule, the operculum and the calyptra, it is impossible to give indications positive enough for directions. The calyptra is deciduous often before the capsule is ripe, it is also ~~very~~ found only with difficulty and ~~then~~ ^{then} by proficient anatomists in small plants. When specimens are collected and mined, the detached calyptras ^{are} generally loose are not always clearly referable to their species. The capsules also are not always found. A great many mosses are collected sterile, no species of Dicranodontium for example has been found in fruit in America. And then the peristome is not only very difficult to separate and to be carefully analyzed; but it is not at all a constant character of the genus. Thus, in the series of Peristome double, 16 teeth of Sullivant, Encalypta has two gymnostomous species and two with an inner membrane or double peristome. In Gygodon all the American species are gymnostomous. In Orthotrichum 1 species is gymnostomous, ⁱⁿ most of the others the teeth are separated by cilia; three have a double peristome; or at least the cilia as large as the teeth ~~happen~~ appear like an inner peristome. And so on. I do not see therefore how a key of this kind might be useful to students except if it is prepared with details of the characters of other organisms and this, of

course will render the artificial they more intricate and difficult to study than
a conspectus. Hence why have two kinds of guides; and why take the artifi-
cial method which certainly turns the mind of the student from a more
natural method. Though that former key based upon the sex of the plants
was very commodious, you would not come to it again. Since Wilson
no work of mines has been published with an artificial key. Mitten has a
conspectus of his tribes and another of his genera in his Musci Austro-Ameri-
cani. Muller has nothing; and if you look over the works of more recent Authors
Lindberg, Braithwaite etc you see in all a tendency to systematically arrange
mines but nothing else. The systematic arrangement is however different in
all and as I find that of Schimper by far the best, the most simple. I have
admitted it. Sharp Watson has done the same. His conspectus is by far the most
clear and commodious kind of key. I wanted to follow it but went too
far with Schimper. I think therefore that if omitting a number of the
less distinct tribes, and abridging the descriptions, I had a conspectus of that
kind, the artificial key would not be needed and that conspectus like
the general character of the mines could be published at the beginning as in Mr
Watson's mines without double kind of pagination. — The Hypoecia especially
should be modified. They represent only two tribes, the Orthothecia and the Campylotheceae.
But it would be necessary to cut them by sections for the Campylotheceae are by genera
for the Orthothecia. Now the sections representing Schimper's genera are all described
by subtribe with as much specificity as are my genera. I thought first to
admit merely the sections. But it was desirable to have a genus after James and
that could not be made in admitting the sections. Schimper's genera are
generally admitted or at least quoted ^{now}. They have been admitted in the Catalogue of
Rex and Harvey. On the question therefore I remain uncertain and indifferent.
If you could direct me what to do, I should like it and follow your advice.
All the recent authors, especially Lindberg and Braithwaite tend to multiply genera
and genera. I am already far back into the conservative ranks; and really, I
think, it would be best to look a little more ahead. — Don't answer this
letter if you do not find it convenient. I shall go on and work to prepare a key
and a conspectus and description and then you may see what is the best
method and thankfully yours L. L. Quereux.

(Columbus O 15th June 83.

My dear Prof. Gray.

I send you herewith the separate list you wished me to prepare, showing the groups, or Tribes and Genera of each Tribe. I have added to it the Sections or Subgenera (how shall I call them) of the Genus Hypnum and the names of the 194 Species described in each Section. That may not be of any use for publication in the Manual, but that may enable you to see the skeleton of the work. I say that the list may not be published. For, indeed, the Conspectus in the Introduction will indicate the pages of the Manual where each tribe is described or mentioned, as perhaps it will not be necessary to repeat a description of each tribe in the text of the manual but only to give the names, either simply or with an abridged description. But, it would perhaps be advisable to record in the Conspectus after each tribe the names of the Genera pertaining to it, as I have done in the pages which I send herewith. The number of the page where the species is described in the text might be added after each generic name or merely the No of the Genus. For the Genus Hypnum, it will be best to give, as Sullivant has done in his Mosses, a description of each section which is

equivalent to that of a Genus. And perhaps it will not be necessary to have the sections described in the Conspectus, except if they are taken as subgenera and briefly characterized. You will have time to see about that untill I come to the genus Hyppium. But now and from the beginning, I should be most obliged if you would fix the question of the descriptions of the Tribes in the text as repetitions or abridged of the diagnoses in the Conspectus or merely mention the names, as Sullivant has done. Mr. Watson has described the tribes; ^{in his key} but neither himself nor Sullivant have described them in the text. Schumacher has them in both Introduction and text. Wilson has them as Suborders, described in a few words in the text. Mitten has Tribes and Subtribes described in the Conspectus with additions of a description in of two or three words with each Genus and serving as Key, but nothing in the text, except a Key for the species of each genus. This is a combination of the Key as I have made it with the Conspectus. That is good for the students, but difficult to make and tedious for researchers. Please! What would you advise me to do.

A second question not clear for me is that of the punctuation. In Mr. Watson's work, in the descriptions of the genera, all the essential organs of moths, Leaves, Flowers, Calyptra, Capsule etc are separated by a period . and their parts by a semi-colon, or a colon. But I can not distinctly see why some

partial organs are separated by a ^{semi} colon like Calyptra; while others like operculum have ~~only~~ a semi-colon before them. In the description of species, the period is replaced either by a colon or a semi-colon and here also I find the same uncertainty of mind about the distinction of the one or the other. In Schumacher, Muller, Mitten, Wilson etc. I find for punctuation only the comma and semi-colon. ~~But~~ in the description of the species, adding the period by some authors ⁱⁿ the description of the genera. You have the same in your manual, though you frequently use the colon in the genera. If it was not too much trouble to you, please give me a short direction if possible.

As soon as I have ^{an} answer to this, I will go along reviewing once more the manuscript to try and have it all right for the printer. I have already arranged the whole according to the divisions ^{and} subdivisions, in the order marked on the pages herewith or the systematic disposition, and ^{will try to} be ready with the Hircampi when the Sphagnaceae and the Andraceae are ready in galley. I suppose the correction of the galley and of a first revise will suffice. Probably the publisher has a corrector who will see for expressions which are not good english. But I may correspond with Mr. Caserio on that matter.

I can not say how it grieves me to be forced to give you so much trouble by my ignorance.

Yours friend very respectfully
S. J. Guerin.

Columbus O. June 20th 83.

Dear Prof Gray,

I do not know how better express or describe the characters of the ducts. They are triangular, placed at or near the concave surface of the leaves, the lower side of the ducts being free ~~free~~ (not embraced by the utricles and thus forming part of the inside or concave surface. ~~CCCC~~ The side of the ducts is in line with the lower part of the utricles but free not enclosed by them. Sullivan says in the mones: that the utricles are triangular and placed between the rotund utricles near the concave surface of the leaves. I did not write that they are placed between the utricles because this is the general character. They can not be placed otherwise. But the ^{utricles ducts} sometimes protrude beyond the utricles or are sometimes constricted and enclosed between them so that the border of the utricles pass above or below and thus I thought my description more expressive and more clear. If you think the contrary please to substitute that of Sullivan as underlined above -

For Luteichia. The leaves are distichous, ~~not only~~

not only very concave but keeled so that they look
as flattened on both sides of the stem. I will modify
the diagnosis and say stems compressed? but
it is the leaves which are compressed or so much
flat keeled that they look compressed.

I should have liked to know about puncta
too. I am trying to make my diagnoses more
clear and to have all as well as possible to
me, for the *Pyceae*. But the *Phrygnae*
have been ready for more than two years, sent
to James to have them examined and submitted
to you & yours. He always said that they were
all right.

Why should James force me to go with
him against my repeated protest? I urged him
to make the work with Austria. He would not
hear of that; then with Mr. Watson, offering him
my assistance, if needed, my notes with illustrations
all I had. He would not have that too. I
will know how much trouble I should have
in the matter with my poor head and language
and now you have to suffer for that too.

Very truly yours

L. Derjucerey

(Columbus O. June 21st 83

My dear Prof. Gray.

Your kind letter of June 18th is now received. But I met you already yesterday evening and this morning too before I got your letter from the mail.

What you say about the possible arrangement with Mr. Watson is good news indeed. If money could help to decide Mr. Watson to help me please to make arrangement with him for that. I will gladly pay any amount he may think sufficient to pay his time and trouble. I will write him about the tribes and all my ideas when your train arranged matters with him. I trust that once in progress the work will go smoothly. But

I can not trust myself and if
the proofs are read once by Mr
Watson, I know I shall be out
of trouble.

Very truly and thankfully.

L. S. Squire.

Columbus 0 June 26th 83.

My dear Prof Gray.

The idea of imposing upon Prof Watson such a hard task as the revision of my mss. or even the reading of one of the proofs is really distressing to me. I have no claim to his time, nor can I understand how I could ever cancel my obligations to him. I do not dare to offer to M. Watson a compensation in money; but could you not do it for me? It would ease me very much if I could acquit myself in some way of what I shall owe to him. I will answer his kind letter now.

I believe however that after the Sphagna and when we have cleared the matter about the Tribes etc. there will not be any more difficulty in the way. But on this question, though I have turned it and reconsidered it on every side, I can not come to agree with you. The tribes as I have fixed them as most of the authors have done before, are distinct groups well prepared by their characters. See for example ~~the~~ Pisidians which has neither the plants, nor the leaves, nor the capsule or the peristome^{of a Weissia}; or Octoblepharum whose affinity is with the Sphagna by the leaves; Compare ~~the~~ ^{also} Grimmia to Orthotrichum differing in the same degree by all their organs and so on. When differences so marked exist, you said it yourself, a genus could not pass as a subtribe of the other; ^{or rather} the Orthotrichae can not go as a subtribe of the Grimmiaceae. As the Weissiae are the most difficult to describe with precision, I could arrange

The series of the mosses as follow.

Tribe 1. *Phascaceae*.

" 2. *Wetziaceae* (subtribes) 1. *Dicranaceae*, 2. *Pseudocentraceae*, 3. *Seligeria*,
4. *Ceratodontaceae*, 5. *Seligeria*.

" 3. *Gottaceae*.

" 4. *Grimmiaceae* (subtribe) 1. *Hedwigiaceae*.

" 5. *Orthotrichaceae* (subtribe) 1. *Encalyptaceae*, 2. *Tetraphydeaceae*??

" 6. *Physcomitracae* (subtribe) 1. *Plachnaceae*.

" 7. *Bryaceae* (subtribe) 1. *Bartramiaceae*, 2. *Mesuraceae*, 3. *Aulaceae*,
- *complanatae*, 4. *Timmiaceae*.

" 8. *Polytrichaceae*.

" 9. *Burbaurmiaceae*.

" 10. *Fontinalaceae* (subtribe) 1. *Cinclidotaceae*.

" 11. *Hypnaceae* (*Orthotrichaceae*) (subtribe) 1. *Neckeraceae*, 2. *Pseudocentraceae*, 3. *Hoo-*
- *keriaceae*, 4. *Fabroniaceae*, 5. *Leskeaceae*, 6. *Tylaiaceae*.

12. *Hypnaceae* (*Campylotheceae*) for the genus *Hypnum* and section.
But with 3 or 4 Tribes more and less subtribes, the distribution would be better.

Or I could make a totally new subdivision in groups as follow
for the *Stegocarpus* or even the *Cleistocarpus*.

Tribe 1. *Orthocarpus* ^{capsule erect, peristome simple or none} Group 1. *Phascaceae*, 2. *Wetziaceae*, 3. *Dicranaceae* etc.
to the *Bryaceae*.

Tribe 2. *Climacopaceae* capsule inclined or pendulous, peristome double 1. *Bartramiaceae*,
2. *Mesuraceae*, 3. *Bryaceae* etc.

Tribe 3. *Fontinalaceae* 1. *Fontinalaceae* etc.

Tribe 11. ^{*Hypnaceae*} *Orthotrichaceae* } These two should make one only with
Tribe 5 ^{*Hypnaceae*} *Campylotheceae* } a peculiar name as yet not fixed; or might
be taken as two Tribes.

This last ^{mode of} division is apparently more simple. But it would necessitate
quite a long description of each group and that would not be even for
mossologists as clear as the distribution into the 12 tribes. Indeed,
as I said above, except for the *Wetziaceae*, the characters of the tribes, as I find
them in the systematic disposition sent to you, are clear, precise and easily
understood.

I wish you could still let me know your opinion on the subject
by a few lines. I will try to correct the work until you find it acceptable.

Very truly yours

L. Dequeren

The first proposal seems to me
more appropriate & easy. - Combining
11 & 12. to *Hypnaceae* it is

then subtribes 1. *Orthotrichaceae*
2. *Campylotheceae*

But probably you had not sent
yourself - I put an explanation to the
covering it in a note to Key,

Columbus June 26th 83.

Prof. Sereno Watson

My dear Sir.

The offer made in your kind letter of reviewing part of the ~~ms.~~ of the Manual of the mosses imposed upon yourself such an amount of hard work and such a great loss of time that I scarcely dare to accept it, though I greatly feel the ~~need~~ ^{need} of your assistance. Could you not enter into some kind of arrangement and accept some compensation, if not for the value of your work, at least for the loss of your time? -

The manuscript of the Sphagnaceae was written long time ago and has been more than two years with Mr. James who had the work of Braithwaite ^{the ms.} I read ~~it~~ however when it was returned to me long time after the death of Mr. James and merely modified some of the quotations from that same work of Braithwaite which had been lent to me by Mr. James. I admit the fitness of your observation in regard to the authorities of species or varieties from a discussion on the subject with Prof. Gray who took the trouble to put me right on the subject,

I have reviewed and modified all the quotations from the beginning of the *Myrica* and believe that you will not find any more errors of the same nature. I ~~have~~ also generally followed your mode of arranging the quotations in the book and reduced their number perhaps too much. The synonymy is of little use for a manual. I have therefore generally quoted the author of the species and the work where the species has been illustrated for the first. Braithwaite's work is certainly very good. But for the figures and the clearness of the description, that of Schimper, *Turfmoor* is better, to my opinion. This one however does not represent as many species as Braithwaite does. I have also received lately, ^{European and American Sphagnum} (two weeks ago) from Lindberg, his *Atlas Braithwaite*, the synonymy occupies a great deal of place. There must be also some excuse in my favor for incorrect quotations in the deficiency of my botanical library. When Sullivant was living, I had his books at my disposal. Since the Sullivants' library was transferred to Harvard, I have had to rely on what I have and that is not much. Thus, for example for the var. of ~~hypnoides~~ *Sphagnum cuspidatum* I find quoted in Muller *S. hypnoides*, A. Braun

in Bot. Zeit. etc. For scientific details of this kind, it is certain that the long synonymy of Braithwaite and Lindb. has great value. And my remarks too are not made in a sense of objection to what you write me, for I have many times felt the insufficiency of some of my quotations, and the necessity of correcting them if possible. For the mosses for example, described by Desimp. *Flas. in Proceed. of the Am. Acad. of Arts & Sci.* I have to quote from a separate copy of the memoir; but I do not know the volume, as I have not the *Proceedings*. This deficiency is lamentable, indeed. I shall be greatly obliged to you for correcting any traces of it. And please be certain of this: that I shall consider as a kindness of yours any ~~kind of~~ critical remarks or corrections that you may find advisable to make on my work.

Very respectfully and thankfully yours
J. S. Sargent

Columbus 10 July 19th 83.

Prof. Watson.

My dear Sir.

Returning the Galley which you have had the kindness to correct, I take the liberty to say that in my opinion the heading, description of the three orders is superfluous, as these descriptions are more completely given in the prospectus and also in the text with the description of the species. For though you think with Prof. Gray that the descriptions of the tribes and subtribes shall not be repeated in the text, it is however necessary to give a short account of the orders, like that which you have put before the Artificial analysis. Should you, however, find that it is best to put this description before the key, I should like to have the descriptions somewhat modified, as in the page here below, which if you find correct could be pinned to the Galley. Your correction is so well made that I have nothing or I care any thing to change. ~~I~~

Is not Racomitrium as good or better as Rhacomitrium, at least on account of the authority. I think Bridell has written Raco Muller and Muller have

Phaco while Milton and Schimper have Praca.
Perhaps the accent & demands h. This I have
forgotten with most of my Greek.

I shall take account of your expenses in stamps,
etc. All that is paid by Mr. James; money.
But I hope that when the work is once started
the publishers can send me directly the galley and
the proof. But I now greatly need your advice
and kind directions.

Very truly yours
L. Serquereux

Order I. Sphagnaceae. — Capsule distinctly
operculate, gymnostome raised upon a pseudopodium
dilated and divided at the summit; columella basilar.
Stem composed of a triple layer of cells. Leaves soft,
pellucid, composed of ~~a simple layer~~ of two kinds of
cells in a simple layer, viz., large, chlorophyllous
porose and fibrous; small, narrow, chlorophyllous,
interposed between the large ones.

Order II. Andreaeaceae. — Capsule etc.

Order III. Bryaceae. — Capsule etc.

I would rather erase Peat mosses, Schistocar-
pous mosses, and True mosses. At all these
terms are repeated from the prospectus and also
put in the descriptive part. I should also omit
the remarks on the color of the mosses etc. in the
Sphagnaceae and the Andreaeaceae, and their habitat.
But these remarks are mere suggestions.

Columbus O. July 21th 83.

Prof. Sereno Watson, Cambridge.

My dear Sir.

When I returned to you the first Gallego proof and wrote you about it on the 10th I had not read your note which remained sticking in the envelope untill I cut it by chance in the evening. Please to believe that I do not object to any corrections which yourself or Dr. Gray find advisable to make in my work. I merely thought that the Orders being described in more details in the systematic prospectus and also in the descriptive part of the manual, the repetition before the key might be superfluous. The same with the explanation of the words of some organs, as all these words used in the descriptions must be explained in the Introduction.

The omission of a ~~note~~^{note} for Sphagnum and for Andræa, and a modification in the series of the Genera, caused by the omission of a number of Tribes has changed the ^{places} ~~numbers~~ of some Genera in the text. I thought first that I could correct the numbering ^{as first mapped} of the key

in reading the 1st Revise. But it is best to have the whole in order at once and therefore I have passed again over the ms. and definitively marked the Genera. I send you herewith a list with the numbers as they ought to be placed upon the Artificial Analysis. If, were there is nothing to be changed in the descriptions of the genera and in their relative place, except for N° 46, Cinclidotus, whose place is more appropriate with the Grimmiae than with the Fontinales. I have also added N° 128, Hypnum whose place must be indicated in the Key in a separate section before the last one C which ends the table. The braced lines of p. 6 sent herewith may be cut and pinned to the galley. If the printer sends me directly the galley, or 1st Rev. I will spare you the trouble of making these corrections.

I have omitted in the text the description of all the tribes. Shall I do the same for the subtribes of which there are a few? If I have to omit them, the description

of each of the following genera has to be somewhat more detailed by the addition of some of the characters, of course. This change may be done easily and in a short time. I must have all in good order before sending the ms.

I feel really ashamed to ask you any question and to request an answer, when you have already so much trouble about that work of mine. All I can say is that I am under great obligations to you and very thankful for your good offices.

Very truly yours

L. Desguereux

Columbus 0 July 25th/83.

My dear Prof. Gray.

Though fearing to be intrusive and to trouble you in writing to you, I am always very pleased and grateful when I get one of your letters. The last one of the 21th. In^d received yesterday is especially welcome as I see that you approve of the insertion of the descriptions of the tribes in the text. With an abridged description of the tribes in the Conspectus, each followed by the names of the genera represented in it and also a short description of them ^{in text} with reference to numbers, there will be thus a more scientific key quite as easy to study and to follow by students as the artificial one. The mere mention of the tribes in the text without description under them would necessitate a constant recurrence to the Conspectus. - Would it not now be advisable to give also with the Sphagnaceae and the Andreaeaceae and in the beginning (not separately before the artificial key) a description of the orders as I have made it in my manuscript and ~~as~~ ^{as} it is done for the Bryaceae and to admit Sphagnum and Andreaea with ^{as} 1 & 2, thus putting the whole in harmony?

I have already rearranged the text of the Bryaceae taking out the descriptions of the tribes as I was formerly directed. But I will gladly go over ^{the most} one more

and fix the matter as it was originally. — As for the number of tribes, you may be sure that I do not admit any more than it is necessary. As you say, Muller has Tribes, proportionally a good many more than I have. Milten has then two in the same proportion ^{of} amount. Sulhrant ~~has~~ admitted 35 tribes and we have now a much larger number of Genera, ^{than he had} some of them like Disculum, Schistostegia ^{being} without analogy with any other, necessitate their separation into tribes. Without separating them into distinct tribes, the mones taken altogether or with the descriptions of the genera only are mixed into a confused, incomprehensible mass.

You have given to Braithwaite's Sphagnaceae a very favourable notice. Now, see how he is proceeding. He omits the Sphagnaceae in the Bryaceae but counts the Andraceae, a queer distinction and ~~for genera~~ he has already 7 families in 7 families: ~~For~~ among them Tetrapleurae, Funariales, Sarcobryaceae, Dicranaceae which are not even admitted as tribes, or subtribes by Mr Watson. Braithwaite follows Schimper ^{for the divisions} and he is right in that; but he follows Lindberg in the multiplicity of the synonymy, in descending as far down as possible to the bottom for quoting the oldest authorities and admitting old forgotten names and this can

certainly not be done for our Manual. See ^{also} Muller with his 23 tribes most queerly mixed, separating Brya and Minium, one tribe for each and putting the Goltuchaceae with the Minium without any reason whatever. Schimper has 26 tribes and a large number of families for subdivisions. This is certainly too far subdividing; but Willson in his Bryes of Britain does not better with his 3 orders and 36 suborders as divisions. — But it is useless to bore you with exposition of this kind. All the authors more or less agree in the number of subdivisions; but all differ also more or less in their mode of arrangement according to their relative affinities ^{which they recognize}. I truly believe that after due consideration you will find that the series of mones as I have admitted and fixed it is the more convenient and the more easily understood by student.

I have sent to Mr Watson the list of the Genera with their N^os as they ought to be given in the artificial key. Now you will please decide if you will have Sphagnum and Andreea counted as N^os 1 & 2 or left out without number for in the first case I will have to remember the genera in the text. If you do not want them the N^os are all right now.

Very sincerely yours
S. Requeyen

Columbus 6 July 1883.

Prof. Levens Watson.

My dear Sir. I return by this same mail the galley proof 2. 3. The numbers are corrected according to the change made in omitting N. 1 & 2 for Sphagnum and Androsace. I wrote to Prof Gray about the matter. For as he found convenient that I add the descriptions of the tribes and subtribes in the text, it may be that he will consider the numbering of these two genera as advisable ^{also}. If I was the case I will have to change the N. 3 again and may do that in the 4th revise. - I answered at the time of the galley your remarks about Weisia and Catharinea. I do not know how Hedberg has written the name. But all the authors I have on hand except Willd. have written Weisia. Sellowart also, and I rely generally on him as American authority. He had a much better library than my own and was very careful in his nomenclature. - I do not admit Catharinea instead of Alchemilla, though the suffix is alike. The name is bad while the other is a true descriptive name in harmony of nomenclature with the tribe. Nor do I wish to follow Lindberg or Brathwaite in admitting oldest generic name - For their oldest generic name are

generally ~~unstable~~ ^{untestable} or forgotten. But names
like ours should, in my opinion, prevail: names
well established and admitted by the authors
more generally known. These change of names
are merely confusing. Brailmont is not yet
known as an author. It will be a long time
before Schimper, Muller, Motter ~~Schimper~~ etc
are forgotten or their names ^{and works} obsolete. - In
this opinion, I follow Prof Gray who wrote to
me to select for our genera good well established
and well known names, without caring about
a nomenclature by precedence (or something like)
And too. I am not in position to follow that
method. It ^{demands} a great library and the
collections of mines, (herbaria) of the old
authors to discuss their nomenclature.

Enclosed remarks, which will
explain you why some names have been
admitted in the following descriptions. I hope
that you will give me your advice for that.

Very truly yours

A. de Quercy

What you kindly propose, to read the proof, before sending them to me would be by far the best and I am much obliged to you. Please, do that. In case of important correction, I should write to you, separately.

Now about your queries on Sphagnum. I can not be very precise. I have no copy of my mss. of Sphagnum. All the documents were sent to James when he began the Manual. I should have rewritten it. But I did not think I had time, as the whole work was demanded for the printer.

S. fimbriatum should be described and I think I had described it.

S. compactum of the Yosemite valley. Bolander is *S. rugatum* var. *compactum*.

S. mollicum, Oregon, Newberry? I do not know that. Where have you seen it?

S. subcundum, Mariposa, has been *S. Lescurei* Tuller. Mon. U.S. but ~~perhaps~~ ^{*S. Lescurei*} is *S. subcundum* again. That species having been named by Tuller and in his Icones. p. 15.

I cannot *S. cribrum* ~~Amst.~~ as a var. of *S. macrophyllum* and will describe it (it is not already described 10?) as a var. *Elvidianum* Austin and quote *S. cribrum* as syn. ~~und.~~ ^{und.}

I have still another new species *S. Fitzgeraldii* examined by two French Mycologists one of them Penzance has made a good sketch for me and also a good description. The place is after *S. Lescurei*. I send that description herewith. As perhaps it will be in time for the printer. Very truly and thankfully yours, D. S. Sargent

Columbus, O. July 31st 83.

Prof. J. Watson Cambridge.

My dear sir.

Your very valuable letter of the 17th is received and for it, I give you my best thanks. It gives me some information and directions which I was anxious to get. I should have come to you (in letters) already many times, but I ~~like~~ ^{feared} to trouble you and to intrude, taking for my own advantage your precious time for reading my long letters and answering them.

What you say about the beginning or the first part of the Manual is all satisfactory. I shall not correct again the Nos. of the Genera nor make any change to their relative position now; only replace (in the text) the descriptions of the Tribes which I cut out. I really think that these descriptions are valuable for the student. Short descriptions of the tribes with also names and very short descriptions of the genera of each tribe will be also good as a kind of key in the Conjectures. I could not in any way find fault with your arrangement and distribution of Tribes and Genera in the Bot. Cat. On the contrary, I wrote to Dr. Gray that it would not be done better.

and as you will see my own systematic arrangement differs little from yours. I have a few more tribes. But it is quite a matter of course as a large number of genera of the manual, some of them necessitating a reparation under peculiar tribes are not found in the California mosses. - You say that the English botanists do not mention tribes and other divisions in the text. That is true essentially for Hooker, at least in his *Synopsis filicum*, a work which for me is very dry and of difficult access by the necessity of running over it, sent by the author from a division to another, from a genus also to another, until I can find what I need to know. In contra the *Bryologist* Wilson and especially Muller have carefully exposed their systematic arrangement, at the beginning by a prospectus and then, for Muller at least by repeated descriptions of the tribes and of the genera in the text, even with the arrangement and short diagnosis of the species after each genus. Braithwaite is a disciple of Lindberg and follows it from the beginning, as far as he has gone until now, at least. I have already written to you about it. Should we admit the modification proposed by Lindb, or admit the oldest known genera, what

should we have? You can see that in his *Musci Scandinaviae* in *Systematia noro naturalis* etc. where *Schistophyllum* for *Trinidum*, *Artrophyllum* for *Mnium*, *Sphaerophyllum* for *Aulacomnium*, *Leucocoma* for *Encalypta*, *Mollia* for *Trichostomum* and *Amsotheicum*, *Swartzia* *Ditrichum* adopted by Braithwaite. That revolutionary system may be good according to some opinions. But I am too old and therefore too conservative to admit it, though so that I should find as many reasons to admit that of Muller, or of Muller which also present some differences. The more one studies the mosses the more he finds the impossibility to arrange them in some consistent natural system and also the difficulty of fixing precise generic divisions or rather of knowing which of the names given by different authors are the oldest or the most right. Hence I think best to stay with Schimper, Sullivant, Wilson, Muller and other authors, whom I have followed for half a century. I have no time to make a new apprenticeship and I truly believe that the *American Bryologist* will support my opinion in the matter. At least they have followed Schimper and Sullivant until now, as you have done yourself. I do not say this to depreciate the worth of Lindberg whom I have in high esteem and who is certainly one of the best bryologists now living.

Columbus, O. 4th August 83.

Prof. Sereno Watson, Cambridge.

My dear Sir.

I return with this Galley 5 & 6. After you have read them, I find nothing to correct. I believe, however, that it is better not to quote ^{N^o of} pages in Schimper's *Tafeln*. There are 2 ed. of this work, one in French, the one I have, of which the text is different. You will see that, probably for that reason, Schimper rarely ~~gives~~ the pages in the quotations of that work. ^{in the synopsis,} I also wish to correct *Squarrosulum* which was a lapsus penae. You will see that Schimper has *Squarrosulum*, this from my mt. & communications in Switzerland. — After studying your corrections in *Sphagnum*, I took again to reading, for the 5th or 6th time, perhaps, my mt., in order to correct punctuation, and other matters which I thought to correct upon the galley, but which I would like to have all right, in order to spare you the trouble you take about this work. — I am not quite clear about the punctuation. I suppose: ~~separate~~ different organs not part of a whole, as stem, leaves, capsule. But you precede by ; the operculum or lid and by ; the calyptra. while I supposed that all the parts of the capsule should be ~~separated~~ by ;. — I am somewhat embarrassed also about the quotation of the varieties. Schimper and others do not quote authorship after the name of ^{the variety} ~~the variety~~, while Brathwaite quotes his name. I should prefer ^{to do} and have done as Schimper did, and did not mention ~~the~~ ^{the} variety after varieties, the descriptions being proofs of authority.

In order that you may see how and where I am not in the right, I send herewith ms. pages 116-119. If you have the kindness to look over a few of them and make any remarks you seem advisable, I will take care that the balance of the ms. be right, according to your directions. I certainly will do the best I can to spare you trouble and loss of time. —

Though I do not admit the modifications proposed by Lindberg for the nomenclature of the Genera, I will however quote them either after the Generic name or rather after one of the species as I have done after *Cynodontium Schistlii* which for Lindb. is *Oncophorus*.

The modifications I had to make in the nomenclature to follow the directions of Dr Gray and your method in the Calif. flor. have necessitated many corrections, erasures etc. I think, however, that the pages are clear enough for the printer. You will find them generally better hereafter as they have been copied by an amanuensis.

Very truly yours

A. Leizner

It would be best if the printer could send me for review a number of pages, as a too small package may be easily lost. I should also like to have two copies of the review one to keep here for reference a comparison.

See on the question of the genera that Braithwaite describes *Fissidens* ^{Hedw.} while Lindberg has *Schwetsophyllum*, &c. Tyl. This last name is older!

Columbus O. August 13th 83

Prof. Sereno Watson Cambridge
My dear Sir.

I think you are right about *Sporobolus*, for
as *S. Beyrichiana* is a true *Bruchia*, that genus ^{*Spalysca*} should
be omitted. But had Schimper not right to take it
for the description of a different species? Are there not
cases of this kind or mode of nomenclature in Botany?
I can say nothing on the subject and indeed do not
know how to mend matter. For the only way
would be to describe ~~As~~ *S. palustris* as a *Bruchia*
as Lindb. has done, or to put ~~down~~ with all the
Bruchiae as *Sporobolus* with Muller. Muller
makes of *Sporobolus* a section of *Bruchiae*. But any
change in the place of a genus is now impossible
as it would force a modification of the Numbers of
Genera in the artificial analysis. Perhaps I would add
a short note after *Sporobolus* or if you should point out
the error by a foot note signed by your name, I should feel
much obliged for it.

I shall be ready this week with the whole mat.
I shall send it when ready all together or by instalments.
The communications by mail or express are often very slow
as it was the case with James. Sometimes I have letters in one
day; sometimes they are three days on the way.
Very truly and thankfully yours
A. S. Sargent

Sturtevant O. 20th August 183
My dear Prof Watson.

I send to day per express the end of the
Acrocarpa. (Mss.). I will have to look again
over the Hypnaceae or the sections of the genus
Hypnum and will send back end of the week.

I will arrange for Sporectores for the modifica-
tions of the characters from those of Bruch.

When I sent for the first time the MS of
the moner, ^{there was} ~~the~~ an introduction expounding the
organs of the moner with descriptions of the names
used for all these organs and their parts and
then a systematic disposition, Tribes, Genera &c.

Prof Gray objected to the printing of these
part in the beginning of the work and wrote that
both should go into an introduction differently
phrased to be printed after the descriptive part.
He said that words of organs, definitions of tribes
&c. could be more precisely fixed in writing
than when correcting the proof or after.

Hence I have now nothing to add. I intend
to give a short description of the tribes and
after each, a distribution with short descriptions

of the genus. I think I met you already
about. ~~Isid.~~

I return the galley received Saturday
night. the 18th, always very much obliged for
your remarks and corrections which are for
the best.

About that Ephemerum habit of Lindb.
I know no more but what he says. He compares
it to E. papillosum det. from which, he says
it differs very little; but says nothing about
E. E. spinulosum. I think that the last
species is rather E. spinulosum Det Schp.
than Schp., as you say. (1)

Very truly yours

L. Lesqueres

(1) Nothing however is said of the species in Bygd.
luc. which is Det Schp. but only in the Synopsis
1st and 2^d Ed. which is Schp. only. Wilson
quote it probably from Schimper, Letter and
may have supposed that ~~the~~ it would be
mentioned in Bygd. luc.

Columbus 10 August 25th 83.

My dear Prof Watson.

In his letter of June 5th Prof Gray writes, after mentioning receipt of the artificial key - "Now will you send too an abridgement of the systematic arrangement of the Bryacea as short as you please, in which we can add the page of the book when it comes. If the characters of tribes and subtribes are given in the body of the book it will not be necessary to give the genera, though it might be well enough to do so." From the underlined sentence (underlined by me), I understood that as the pages had to be added to the description of the tribes as reference, this systematic arrangement could not be printed before the end of the book and therefore should be put as Schimper has done in the introduction under a proper pagination. And also I thought, that as the artificial key of the genera ^{did} expose the ~~at~~ genera characters, it would not be proper to present them again in the same part of the book. - But I have from the beginning recognized that your views resulting of long experience and careful observations are far superior to my

own and I will immediately prepare
that synopsis of the fishes, adding to it after each
fish the name and No of the Genus. I
will try to send you that on Monday next
or at least on Tuesday the 28th with the balance
of the descriptive part.

I thank you again and Prof Gray too
for your interest in the work, deeply regretting
that trouble gives to you.

Very truly yours
J. Deshayes

Alum. b. O. 30th August 83.

Prof. J. Watton Cambridge.

My dear Sir.

To follow the suggestion of Prof Gray, as far as I was able, I have prepared the systematic disposition of the Bryaceae by short descriptions of the Tribes and shorter description of the Genera of each tribe, with the N^o of order. Perhaps the last part, or the description of the genera is superfluous. But if you think so, it will be easy to trace a line around the Tribes or the parts to be printed—I can not really say what is the best. The systematic distribution, as it was originally made, somewhat more detailed than it is in this ms^t. seemed to me sufficient. But I do not trust to my judgement. I had of course some work to do for that preparation and could not send the pages before to day.

I also return the proofs pp 3 to 14. You have on p. 15 a note for shortening three lines. I do not know if that is for me or for the printer. In any case, I have marked with pencil the words which could be left out for shortening sentences, if necessary. If the remark is for the printer these lines are—

to be erased if convenient.

I have added to the list of the works to be consulted for the study of Sphagnum, Huston's, Sphagnologia. It is a pamphlet of 15 pages and 4 plates, badly made. The work may not be of great value as the essential documents are taken from Schimper, Breckhoff, and Lindberg. But Huston is the editor of a Revue Bryologique and his report on the Manual may help the sale of it. He is moreover a very good bryologist.

I think I will read once again the last part of the Musci, (Pleuriscopi). I find so great exactness in your corrections that I feel certain to have always left many errors especially of punctuation, ^{and am ashamed of it.} I may send it next month.

Very truly yours
A. Desqueunes

Columbus O Sept. 3^d. 83

Prof. S. Watson, Cambridge.

My dear Sir.

I send per express to day the last pack of the Manual, the Cladocarpus and Rhizocarpus. Though I have read the ms. again, I fear you will still find too many matters for correction.

There is top of the pocket a page 120 *Frondosities* which I have found among my papers. I can not recollect if it has been copied. Perhaps it may have been left out by mistake or forgotten after it has been corrected and glued. Near, oblige me to see if this page is already in place or if ~~it~~ it was left here by mistake. In this case of course you will please give it to the printer. If copied. please throw it in the waste basket.

Very truly yours

V. Sequevum

Galley 11 & Next pp 17-33. just received. Will try to return all to morrow or the galley this evening. I do not find galley of *Sphagnum Fictygeraldi*.

Columbus 6 Sept^r 17th 83

My dear Prof. Watson.

For about two weeks I have not received any proofs for correction. And also, I do not know if you have received the bal^y of my manuscript, the *Americani*, which I sent per express on the 3^d Inst. I am therefore somewhat uneasy about the matter and if you are at Cambridge and well enough to visit, I should thank you much for a few words, especially to say if the manuscript is in your hands.

Very truly yours

A. Lesquereux

Columbus O 17th Oct^r 83

Prof Watson Cambridge.

My dear Sir.

I was very glad to see the proof coming again. From this, I see that you have come back to Cambridge and I hope that you are now quite well. I hope also that the hard work you have undertaken for the correction of the ms. & proofs of the manual will be now somewhat less arduous. I trust it at least very much.

The last page of the review is wanting in the old proofs which end with the *Obscure* while in the new arrangement the last page includes *Phuridium*. That is not matter, except in case of forced reference.

Please give my best compliments to Dr Gray. The death of Heer affects me very much. I am now the only left of a long row of Swiss friends and colleagues. Heer was 24 years old, three years my junior.

Very sincerely yours

V. Desguery

Return proof by this mail.

Columbus Nov^r 5th 83

Prof J. Watson Cambridge.

My dear Sir

I return to day the proofs which were received only Saturday afternoon, one day after yours of the 31st past.

All the changes you find proper to make are acceptable to me. I am so far behind you in all what concern the preparation of matters for the printer that I merely consider ~~my~~^{my} great indebtedness and obligation to you, for the careful review ^{which} you made of my ms and of the proofs, freely admitting that without your assistance, my work would be very deficient indeed.

The arrangement you propose to follow for the order of descriptions is quite right to my opinion, except for the *Calyptra* which you describe first and which I consider an unimportant part of the capsule, on account of ~~the~~ its fugaciousness, and of the similarity of its character in all the species of some genera. Thus it has been described the last by Sullivan and many authors. It is not a real part of the capsule; but like the shell of the egg it is the first thing seen (when one can find it) of a worm and on this point of view the first to be described. Please to follow your opinion in that matter.

That blunder of mine about the wrong position of *Gimnura* ^{in the synopsis} is a very bad one and I regret it

very much. I hope that the leaf is not yet stereo-
typed and that the error may be rectified. I would
never have found out that mistake. It would
be well to add on the same leaf p. 29 for *Eustichia*:
Peristome none, as marked in infl. That is in discrep-
ancy with the character of the *Ceratodontes* of which the *Peristome*
is indicated with 16 teeth, and perhaps it would be
advisable to add before (Subtrib. *Ceratodoeae*) *Peristome*
none in *Eustichia* and then nothing more should be
added to the character of *Eustichia*. For this, also
please to do as you think best. - You will find
herewith a description of *Eustichia Norvegica*, as made
from the specimen that Miss Hought sent the kindness
to send me and also from her description in Bot Club.
This discovery has given to me a great pleasure. I have
searched those fruiting plants for 30 years or more.

About *Brauneria*, I do not know what to say.

We do not have any species of *Brauneria* - and I admitted
that subdivision *Pseudo-Brauneria* only because I did
not wish to make a new ~~genus~~ ^{in the American moss} when we had already
three which could be ascribed to the same genus with
little modification. If you consider ~~the~~ best to put
Brauneria in the key p. 49, I am quite satisfied
with the change.

The synonymy of *Dicranella crisp* & *D. Schreberi*
may be right as described by Lindb. & Braithwaite.
But *Dicranella crisp* like *D. Schreberi* have the
leaves like those of *D. Grevilleana* spreading all around.
The question is only, I think about the reference to plants
named by Ehrh. and Schreb. Admitting that nomencla-
ture we should have *Dicranella crisp* Ehrh. and
Dicranella crisp Schreb. in the same section.
I prefer to leave the question to future Mycologists.
In erratum *Anisothecium* after the first section of *Dicra-*
nella and ^{leaving} ~~resting~~ the synonym as marked ~~and~~ ^{at} p. 64
the matter is clear. But certainly *Dicranella crisp*
is to be placed in the section of Leaves squarrose or spread-
ing all around, though a *Dicranella*.

Many many thanks for your great kindness.
I am really distressed to see how much trouble you have
with the correction of the list and the proof and how
much time you have to give to that work.

Most sincerely yours

W. S. Sargent.

Is not it a rule to put as heading the page the
generic name last marked or described upon it. Hence
should it not be *Trematodon*, head of p. 62 instead
of *Dichodontium*?

Columbus O 8th Nov. 83.

Prof Watson.

My dear Sir.

I know I am always in the wrong! I have corrected matters. Perhaps that will do. Only I do not know if Willson is right in supposing that the cellular matter comprising the operculum is the top of the columella. - Perhaps it would be best to leave out that ().

I see by the *Revue Biologique* that Braithwaite has shelved part of the *Dicranocera* out. Probably that will necessitate some change in the synonymy. I will be obliged to you for any correction you may find advisable to make, as I thank you also very much for returning that page which you will find herewith.

Most sincerely yours

L. Lesquereux

Columbus O 12th Nov & 83.

Dear Prof. Schimper sent us a new species under
the name of *Dicranum pallidum* a specimen which
Sullivant & myself recognized as our *D. spurius* which
is very slightly different from the European form.
That is No 85 of the Musci. For the other, the
synonymy should be *D. condensatum*, Hedw as
in No 86 of the Musci, erasing var. *condensatum*
or leaving var. *condensatum* (as of the authors) without reference.
which is only indicated upon labels in Sullivant's
herbarium? or at least in my own and as you say
can not be mentioned.

Very truly yours
A. DuRoi



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE



Prof Sereno Watson
Botanic Garden
Cambridge
Mass

Columbus, O 15th, Nov. 83

My dear Prof Watson.

Trichostomum tenue Schrad. is *Trichodon*
cylindricus. Schp. or *Trichostomum cylindricum* Hedw.
Trichostomum tenue Hedw. is ~~an~~ hypothetical or
rather unknown. Schimper 1st Ed of the syn. referred
to it under the name of *Leptotrichum tenue* a Southern
(mediterranean) species now *Leptobarbula herica*, ~~ackn~~
providing his error in syn. 422 p. 189. That *T. tenue*
Hedw. has not been seen by any body. Muller describes it
from Hedw. and from Schp. syn. 1st Ed. Lindb. does not
mention it and as Schimper says the moss is non rite
cognita. Neither Sullivant, nor myself have seen anything
that could be referred to Hedw.'s description. Hence, I
thought it would be best to leave it out. Perhaps
it would have been best to have put a note after the
X: 41. description of *Leptotrichum tortile*, like this.

Trichostomum tenue Hedw. is as yet an uncertain species
of which nothing is known but Hedw.'s description. According
to the description, it differs from *L. tortile* by its large double
annulus, the more solid brownish red capsule and the
plane margin of the leaves. If you think proper to add
this, you will oblige me.

Over

About Trichostomum Coloradoense Aust.? I can
recollect nothing. I do not find it in my
catalogue where James has indicated all
the species he thought I had forgotten. I do not
find it either in my 1st manuscript. It is one
of those numerous species of Austin which made
from sterile specimens may be referable to different
genera; ^{and whose description is more embarrassing than profitable.} ~~and~~ If we have not had a specimen
for examination, I can say nothing about it. If
however it is not described, I should like to ~~give~~
Austin's description, as it is not well to omit any
thing from him. If James had seen the specimen
he should have and certainly would have mentioned
it. The Mr. James who gave the specimen to Austin
is probably not the same friend? In any case, as
I say above I do not recollect to have seen any thing
about that species.

As we have paid the Dicranes, I do not
want to see now Braithwaite's Dicranes ~~W. & A.~~ Will
take the liberty to ask for them later.

Very sincerely yours
L. Senguer

Columbus Nov^r 16th 83.

Dear Prof. Watson.

It is not you who should ask to be excused, but me for the trouble you have with our mosses.

I think also that *Barbula revoluta* should be left out. In my notes of north the connection with Sullivant, I have referred with ? to *Barbula revoluta* a moss found at Brattleborough by Frost referable or rather related to both *B. revoluta* and *B. gracilis*, but differing from both by characters described in note. Neither Sullivant nor myself could decide, the specimens being unsatisfactory.

When I have taken that *B. revoluta* from Califormia, I can not recollect ? It is not marked upon our

Catalogue. It must be one of my blunders. Or perhaps, I may find explanation in some letter of James. In the meanwhile, please erase it. I am only too glad that you take the trouble to look over the mosses with so much care. James could never have done that. - About *Austro-*

Trichotomum coloradense and other undeterminable species (for I consider undeterminable any kind of Herk Trichotomum, most *Barbula* etc.)

would it not be best to leave out as uncertain
species those of that kind and put them at
the end of the volume as a kind of appendix?

All the species of Austin which have been examined
by Sullivan, or James or myself we know what
they are worth but there are some of which I
never saw a sketch and which we know only
by Austin's descriptions. They may be all right;
But I do not know about that. The number
of these uncertain species is not large.

Please do not spare me your remarks. The
more the better and I fear you will have plenty.

Very truly yours

A. S. Querry

Columbus, O. Nov^r 23^d 83.

Prof. Sereno Watson Cambridge.

My dear Sir.

Far from being displeased for your corrections of the Manual, I am every day more grateful for the careful attention you give to the preparation of the work, either in reading and correcting the manuscript or in reading again the proofs. I expected, of course, to be able to correct the proofs; but I see and have seen it from the first, that at least for the quotations, references and synonymy, I should have blundered all along and my attempt in that way would have been a failure. I see well how much of your precious time you are forced to give to that publication of the manual and how heavily are the obligations I owe you. But of this I shall speak in time — Perhaps the quotations of authors are sometimes too numerous or ~~do~~ in too great detail for a manual. At least if the manual of moss is compared to that of the Phanerogamous plants by Prof. Gray. But I do not see the possibility of reducing quotations, though I do not wonder them as of great value for the student. In this, you have great experience and I am satisfied that all your corrections are for the best advantage of the book.

I do not know what to say about *Austin's* *Trichostomum Coloradoense*. There are a few others

like that which I may have omitted. But generally I have recorded Austin's description, even agreeing my opinion, adding however a few remarks which I thought advisable to give. Austin has been against us, especially against James from the beginning. and for that reason I have been carefull to say no-
thing ^{against his word} or omit nothing which he has published, when there was no positive evidence of error in his determinations. We have thus reviewed his species as often as we could get specimens and I believe that every biologist would thank me for having omitted all those so called species which he ^{has} made from insufficient specimens. Descriptions of that kind are discouraging and more or less injurious to science. They give trouble without profit. — I now see the leaf I send you. It is a description (translation) of *Trichostomum Coloradoense*. You may either put it after *Trichostomum* or keep it for that short appendix to be put end of the volume for uncertain species.

Most obliged and sincerely yours
L. Vergerney

I send proof per mail with this letter.

7 My dear Sir. Columbus O. Nov^r. 30th. 83.

Schimper. Syn. Ed 2, p. 484 describes *M. riparium* ¹⁸⁷⁴
and remarks: - ~~Fructus~~ Specimina quae sub hoc nomine
e Suecia et Norvegia accepti nullo modo a *M. orthorhyncho*
differunt. - and after. - Fructus rarius reperiuntur; plan-
tae masculae contra, suis locis copiose proveniunt. Non
nisi statura humilior, gracilior, foliis minus longis
paulum laxius areolatis, capsula molliore a *M. orthorhyncho*
differt and non speciei propriae. - Char. Lindberg plantam
hanc sine ratione cum *Lycopodioidi* commutavit, tam statu-
ra multo pucriore, quam capsula longiore and subarcuata and
prospernis sporis dimidio minoribus distincto. - Schimper
is right. It should have been foliis longioribus angustioribus
rigidioribus etc. - Both species *M. lycopodioides* (not rare)
M. orthorhynchum (very rarely) ~~are~~ found in Am. We have never seen
anything like *M. riparium*. Very truly yours A. Disque
The European *M. lycopodioides* is far less out!



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Prof Serrano Watson
Botanic Garden
Cambridge
Mass.

Columbus O. Nov^r 26th /83
Prof. Sereno Watson Cambridge

My dear Sir.

I shall never complain of your critical observations. The more you send the better. My best thanks again for all them.

It is right to add Newfoundland as locality to *Urtica phyllanta*. As far at least as Newfoundland may be counted with the American Continent. But as we have species from Greenland, we have also to consider those of Newfoundland.

Adding your doubtful forms of *Orthocentrus anomalum* from Nevada is also right.

On *Orthocentrus*; I say at the end of my note of it ~~that~~ ^{and on} some of the specimens of *O. speciosus* that some stems of the species which greatly vary according to local distribution, are entirely covered with axillary gemmae, the var. *polycarpum* in Herbarium. But I do not think that I have described that variety, as it should have been.

I have sent to me and to Sullivan a number of specimens under the name of *O. leiocarpum* Sullivan mentions in Herbarium (according to

and that is mentioned in my note too,
James' letters, that all these specimens are
varieties of *O. Speciosum*. It would then be
advisable to add as another syn: of that species,
O. leiocarpum James, ^{inst.} - But there are
a large number of these species of James which
have been determined by Sullivant and by myself
as referable to other kinds, and I did not find
advisable to quote them.

Orthot. *Ohioense*. Sulliv. *Sedg.* in *Scour* is one
of my blunders. It should be *O. Canadense*. This
O. Canadense was made by Thompson from mixed
specimens; its characters are not represented in
any of the Am. species. The species should be omitted.

O. Rogeri was made according to Bryot. *Ill.*
~~a~~ very poor figure and description. It is *O. pallens* as
re-described with a new plate in *Suppl.* to *Orthot.* where
Schenker names it *O. (pallens) Rogeri*. I
think *O. pallens* should be preserved, at least we
have the species as described and figured by Schp. & Sulliv.
aut. and *O. Rogeri* should be added as syn.

Specimens communicated by James with sketches
of *Gummia elongata* & *G. atrata* are referable to
G. unicolor, *Grev.* James acknowledged the reference
as right. No *G. elongata* has been added to my
catalogue by James. None of his species has been omitted
or changed without his acknowledgement.

I hope that the proofs sent and returned to you on
Friday have been safely received.

Very sincerely yours,
A. Sengueren

After making the above, I have just received your welcome note
of Nov. 23. - I have Sanderling *manipula* James but there is nothing on
Indulium subrotundum - But I find that species is *Obso.* *Musci* *Scour*
(1867) and there he does not mention locality of Greenland, but only Scandinavia,
Norway, and Lapland. There is probably a new ed. of *Thomp. l.* I add here
with the description of the species a genus in the *Musci* *Scour*, much
mentioning the essential differences separating the species from *O. Virginian*.

dsy

Columbus Dec^r. 1st 83
Prof. Seewee Watson Cambridge.

My dear Sir.

From my last note which you had probably not received when you wrote me about *Orthotrichum Canadense*, you have seen that indeed the only blunder is that citation is the Supplement to the Journal of O. Ohioense Sull^{vs} Sledge instead of O. Canadense. It is not Sullivant who has made the blunder but myself.

Your statement for authorship of *Orthotrichum* and synonymy is right. But for the accompanying explicative paragraph, it is not necessary to make it so detailed, I think. What you say in the beginning is sufficient. For we can not forget that our work is a Manual.

4 *O. Canadense*, Bruch & Schimpf. [London Jour. Bot. ii 667] remains a doubtful species. It was based on Drum. Musc. Amer. 149 & 151. and is from the examination of the specimens of ^{the} ~~the~~ sets of Drummond, ~~Dr~~ Sullivant failed to recognize the character indicated by the author of the species. I get something like what I could write upon the proof but what I ~~would~~ ~~be~~

I would be much obliged to you for writing
it yourself or most probably I should forget
the matter.

We have discussed the question for this and
other species many and many a time with Sul-
livant. I had first charge of a revision of
the genus *Orthotrichum* when Sullivant was
preparing the material for the supplement to
the *Diagnos*. After up months of constant and
too hard work on this genus, my sight failed
totally and I was forced to quit using the
microscope. Sullivant took the genus after me
with my notes specimens etc. and worked at
it a whole year longer. I was then in constant
intercourse with him and of course ^{we had} frequent
occasions for discussion. No genus of ^{the} mosses
is more difficult and even now less clear for
the true characters of species than *Orthotrichum*.
You will see this in the numerous notes on species
on Sullivant herbarium, notes all carefully
written after the long preparation of the specimens
for his tips and certainly Sullivant will know
the genus far better than Schimper or any living
bryologist.

Very much yours
D. Sargent

Columbus O Dec^r 26th 83

My dear Prof Watson.

What you propose about *Fontinalis Lescurei* var *gracilescens* is the best, though indeed that var. is what Lindb^h separated as *F. Sullivantii*. It was first considered as a forma tenuis of *F. Lescurei* by Sulliv^t and though he accepted Lindb. *F. Sullivantii*, in letteris, he could not, when he came to have it figured for the Icones, find possibility to separate it from *F. Lescurei* which indeed is already too close of *F. disticha* for a species. Hence leaving as you say *F. Sullivantii* Lindb. without the synonymy, var *F. Lescurei* var *gracilescens* or quoting the var *gracilescens* aff *F. Lescurei* is all we can do, Lindberg having of course the property of his species. As said above, he had it figured for the suppl. to the Icones but had to abandon it.

Best thanks. Hope you had a pleasant Christmas. With you a happy new year and remain very sincerely yours
A. Lesqueray

Columbus O. 7th Jan'y 84

Dear Prof. Watson.

Nothing escapes your sight. Your precision is really wonderful!

That *Fuchsiastrum canadense*, as synonym of *Racomitrium lanuginosum* is quoted by Muller, as described by Richard. But I know nothing more about it, nor has it been remarked by Sullivant to my knowledge. The quotation you propose is advisable, but is it not best to say Rich. instead of Michx?

Can you quote as ~~xxx~~ author the one who has given the name of a species without description? Perhaps yes; as Schimper quotes the name ^{wrongly} given to the plant ^{by Sullivant} by Sullivant. ^{*Cryptaea glomerata*;} It seems, however, that it is not enough to give a name and thus merit the right of authorship. You will know best. *Cryptaea nervosa* Hooker should be accredited to Bruch & Schimper.

Very sincerely yours

A. N. Sargent

I return proofs 284-300 to day.

Columbus O 30th Jan'y 84.
Dear Prof Watson.

In answer to your kind note of the 22th.
I have only to say that the proofs not being
forthcoming, I supposed that there was some
good reason for the bill. Well knowing that
under your direction, all is going forward as
favourably as possible.

About Chyromitrium hians, Lindb. — I have
never considered that species as a good one, and there
is good reason to admit C. Hookeri, instead
of C. hians as Lindb. himself makes it
a synonym. In that case also Gymnostomum
turbinatum Michx. should be given as syn.
according to the note of Lindb (p. 51 foot note)
But we have not seen the specimen of Michx. and
should we have seen it, we would have con-
sidered it as mere var. of ~~the~~ C. pyriforme; for
as said above, neither myself nor Sullivan have
found sufficient reason to separate that C. hians,
as seen in the Mus. canic.

Funaria hygrometrica is Linn. in Schp.
Syn. and after that Hedw. et omnes auctores
recentiores. It is also Hedw. in Muller. Herzog
being the first author in this century, I suppose.

Funaria microstoma is Bruch & Schp.
neither Schenk nor Schneck, at least to my
knowledge. Possibly the writer has written
Schenk for Schimp. or I may have made
that mistake myself. But Schp & Bruch
are the authors of that species.

I will see about the habitat of *Bartramia*
formosa, as I do not remember what I
have written.

With best thanks for your note,
I remain as ever Very truly yours
L. Lesquereux

Columbus O Jan^y 8th 84

My dear Prof Gray.

Allow me to write a short answer to your wellcome and kind letter of the last day of past year.

All what you say of your much needed assistance and hard work in helping the preparation of the manual of the mosses is so true, indeed, that I could and would say on the subject much more than you do yourself. It is the sense of my obligations to yourself and to Prof Watson that has induced me to write my last letter to you. and I should have been very glad if you could have proposed some means by which I could be able to repay, partly, at least, my heavy indebtedness to Mr Watson. I can offer nothing to you, for I well know that you would not accept any thing from me as a compensation, even only of your lost time. The world is not yet ready. Perhaps before we are out of it, you will have opportunity to see in what way I could show to Mr Watson

how much I feel myself indebted to him. For, time is money, for him as much as for every worker and I do not see that it would be right for me to accept without retribution so much of his time as he has to give for a work to which he is not directly interested. It is for that reason that I proposed and still propose to have Mr Watson as a third in the authorship of the book and to pay him one half of what I have received from Mr James for the property of the work.

It has been bad for me that I never had a better schooling than that of friends and advisers who did not have that precision of mind so valuable for studies of natural history. I never thought of any higher authorities on mosses than Schimper and Sullivant. And certainly, the manual of mosses after the addition of the synopsis of the genera and the key advised by you, was as good for publication as is the synopsis of Schimper or the Mosses of U. S. by Sullivant. There is even less

attention given to the synonymy in the Jones and Schimper's synopsis than in my ~~note~~. But now, I see how far I was of what is needed ^{in order} to be right and I feel, every time I read proof, more obliged to yourself and to Prof Watson for the assistance given to the publication of that work which will thus become not only valuable to student but honourable to science.

I am obliged for the discourse of Prof Williamson of Manchester. But I have it already twice. I owe him the communication of a copy of all his works. and he has the kindness to send me his memoirs or publications in pamphlet form.

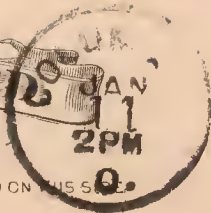
Very sincerely and respectfully
your old friend
L. Suess

Columbus O. 10th of 84.

Dear Prof.

Bryum Bogelowii is a Webera.
I have described it the last species of the
Genus, at least it is so in my 1st list.
If not there please to let me know. It
might have been forgotten by the copist.
and also by myself. Very truly yours

L. Desguereux



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Prof. Sereno Watson
Herbarium of Harvard
Cambridge
Mass.

Columbus O. Jan'y. 12th 84.

Dear Prof Watson.

We have sometimes spoken with Sullivant of that *Cryptaea inundata* ? Nees. and remarked on the impossibility of referring it to something known, at least from the characters indicated by Nees. Nobody has seen anything like that. and it seems that such species are rather to be omitted than described. That was Sullivant's opinion and my own too. But as it is described in the *Morer* of U. S. perhaps it would be best to copy description and remarks of Sullivant. Please do it if you think proper. I add this description herewith.

The terms are much mixed in my mind. I do not understand well the difference between sections and subgenera. I think that the term section might be better appropriate to a large number of subdivisions of a genus like Hypnum. Though it may be, it is certain that a key of them would be of advantage and I will prepare one immediately and send it to you in time.

The glossary is not begun yet. I asked to

Prof. Gray if such a glossary would be
advisable but I had not any answer to that.
It can be done in good time, if you advise
me to do it.

I am much obliged to you for preparation
of the Index. I had begun that in reading the
proof. But I am sure I could scarcely do it without
blundering some.

About a 5th plate, I do not think it would
be possible to do it. We could not have figures
for the sections of the Hypnum and for the
few other genera not figured, the omission is
not important as such small figures as those
which must be made could not represent the
characters in a sufficient manner to be of value
to students. - But besides, I have nobody here
and know nobody who could make the drawings
from anatomical examinations. I thought that
James perhaps could do it. I am unable now
to use the microscope even for a short time.
But as Schimper has copied in his Synopsis many
of Sullivant's figures, I could make a good plate
from cuttings of his figures if yourself and Prof.
Gray consider the addition advisable. Then let me
know as soon as possible. I suppose the engraving would
take a long time.

What you have the kindness to say con-
cerning my letter to Prof. Gray and my propo-
sition of your connection to ~~work~~ work as
author has deeply impressed me. If you
will not accept any remuneration for the
great deal of time you have ~~given~~ ^{devoted} to the prepa-
ration of the manual, I feel myself really ^{burdened} ~~weighed~~
with an ~~extra~~ ^{extra} more troublesome to my conscience
that if it was a debt of money. Gratefollness
of heart and the warmest promise of thanks will
not compensate the slightest part of the time
you have given to the preparation of the Manual.
That I have not been paid for my long years of
research in money is no reason applicable to you
as I worked for my share in scientific pursuit
or for myself. I hope therefore that you will
still allow me to come again to the subject soon.
- what later. What James could have done for
the correction of the proofs and the preparation
of the book for the printer would have been nothing
at all except under your's and Prof. Gray's direction.

Very sincerely yours

L. Sargueren

Columbus 0 Feb'y 4th 84

My dear Prof Watson.

In the last revised proofs sent to me pp. 2112-2125
I find still less to correct than in the ^{before} last ones. The
following are scarcely worth mentioning.

p. 246 line 9; upwards ~~is~~ for upwards

p. 267 " 8 from base; conical-cylindrical, for
conical, cylindrical.

274 " 10 from above - simple, narrower, erect,

Very truly yours

L. Sauer.

My dear Prof.

Columbus O 7th Feby 84

No. I did not know about the localities you mention in Oregon and Montana for *Barthramia* *horniformis*. If you have not yet sent revision. please enclose my note and substitute the localities: Oregon, (Nevins, Hall); N. West Montana (Watson). If I get the revision, I will attend to the matter.

Please excuse me.

Very truly yours

A. Sengueren



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE

Prof. Sereno W. Hutton
Botanic Garden
Cambridge
Mass

Albany 11th Feby 84.

Dear Prof. Watson.

I return 2^d proof pp. 316 & 336. and
keep Review pp. 182 & 194 where I find nothing
to correct, except perhaps p. 186 where the
heading should be, I think, *Tetrapihis*, instead
of *Syraxopodon*. and p. 191. *Tayloria*, instead
of *Tetraplodon*. The heading being *Tetraplodon*.
does not seem right with the 1st species of p. 191.
which is *T. tenata*. If there is a rule for that
I do not know it hence please excuse if I am not
right.

You will see by a pencilled note that, indeed
we have not seen as yet any specimen of *Bostranus*
formiciformis from the Pacific coast. From Japan
and Bering's strait, yes! I believe that if the mon-
itor were, Bolander and Hall would have found it
in California and especially in Oregon.

You were right. That name is Schneck.
as marked in our first catalogue.

Very truly yours
G. S. Vesper

I have not got review of pp. 175 incl. to 181. I should like
to have them as I read the review for the Glossary.

(Columbus 0 Feb'y - 16th 84
My dear Sam.

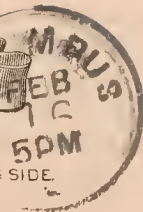
I return to day the two packages
of proofs received this Morning.
Hope you will receive them all right.

Very truly yours

A. A. Noyes

Will wait next week and send the analysis.

07 Hygnum —



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Prof. Serenus Watson
Botanic Garden
Cambridge, Mass.

Columbus O. Feb. 23rd 84

Prof. J. Watson Cambridge.

My dear Sir.

The best is to abandon that N. 172 which is so poor a species that I admitted it with reluctance; like many others made from poor sterile specimens which rather serve to embarrass without the slightest advantage. — I also think you are right to take up Spruce's name *H. turkophorum* for *H. orthocarpum*.

I send herewith a kind of systematic distribution of the subgenera of *Hypnum*, or rather a skin of it; for no systematic arrangement at least none well defined can be made of the subgenera or subdivision of *Hypnum*. I have tried hard and done the best I can; but you may find that insufficient, and if you will kindly suggest amendments, I will try again. — I will send Monday or Tuesday next the plate VIth which I ^{have} made here partly copied from Schimper, partly from the Icones. Two genera, *Thelia* and *Stergonium* being introduced, Schimper's plate could not be used for photography. The plate, I think will be satisfactory.

I am more and more grateful for your kind offer and the care you take in preparing and correcting the text.

Very sincerely yours V. Degeer

Columbus O. Feby 26th 84
Prof. Watson Cambridge.

My dear Prof.

I am the more disposed to make the correction you propose in considering Hypericum pallescens as a mere variety, that I have always asserted the fact, against Austin & James who both sent me under that name a number of specimens all clearly referable to the common and variable H. reptans. But as I have not taken to the microscope again and have not seen Schimper's specimens, I had no right to discontinue the species, though now satisfied that it has been ^{rightly} done by Lindberg. I am much obliged for the correction, more and more admiring the depth and care of your researches. When you call yourself stupid for having missed the correction of one letter, what name will you apply to me who have been and ~~am~~ still blundering along ^{and madding} ~~mistaken~~ without number. I think you will find more to say in the Pleuriscarpus.

Very sincerely yours

J. Derjucerev

Columbus O. 26th Feby. 84.

My dear Prof.

In the revise p/p. 221 to 243 rec^d. this morning,
I find nothing to correct but the few unimportant
modifications which I should correct, perhaps un-
rightly. I write them, however,

p. 222. last line. shortly - pyriform - instead of short.

p. 229 line 14th; p. 233, line 11th; p. 241 line 18. Chestnut-
color, for C. colored.

p. 230. B. provinciale, Hal. - Not rare in Florida,
for Florida (Garber)

p. 231. 1st line. Did you not write formerly - Crypt.,
instead of Musc.?

p. 234 3rd line. The first sentence, if you cut it after
given character by a © has no verb and not no-
minative subject; the punctuation should be / after
character. (I think)

p. 236 ^{line 7} after apex, is or © This form is.

p. 240 line 17 : after Dioecious.

p. 241 line 3. ovate, concave - for ovate-concave.
That is not worth while correcting. But you
leave me nothing to do.

I returned 1 revise and 2 p. of manuscript this
morning. Hypnum compactum, Muller. was sent
me from ^{an} European ^{botanist} with a very good precise description

as a new species, Hypnum Barberi, from
Utah. You found it in Nevada. Most of the
American mosses sent to Europe not determined
are generally considered as new.

Very truly yours

L. Dequereux

Columbus O 28th Feby. 84.

Dear Prof Watson, Cambridge, Mass.

My dear Sir.

I send herewith plate VI, which, I hope you will find satisfactory. Two of the genera, *Thelia* and *Sterogonium* were not given in *Sullivan's Mones*. They are more important than any subgenus. Of them, I have selected from Schimper those more easily recognizable. Nothing now will be needed for engraving the plates but the change of the name *Hypnum* Tab. V into that of *Brachythecium*. The plate is drawn in ink with pen and can therefore be photographed for the new process of engraving, I suppose. But of this I know nothing. Prof Gray will have the kindness to see about and let me know about the bill which I will pay when received.

Very truly yours

L. Lesquereux

The glossary will be ready in time

Note on *H. gracile*. Brit. Schimp.

To be accepted or modified by M. Watson.

This species is extremely variable, according to its location. *H. pallens*, Lindb. n. sp., has been described by Schimper "Syn. Musc." (1822), 611, but ^{it} merely differs from the normal form of *H. gracile* by the leaves narrower, longer acuminate, those of the branchlets more papillose and sharply serrate at the apex. According to the same author, *H. varium* of Drummond's collection (no. 140, 141) found near St. Louis is intermediate between this form and *H. gracile*; ^{and is considered by Sullw. and as a variety of *H. gracile*.} while *H. virginianum*, Brit. ~~clearly~~ ~~described~~ *Hymnium* Dill. tab 36, fig. 18 is recognized by Lindb. as *Thuidium lancastriense*, Aust. ~~the~~ ~~var. *lancastriense*~~ ^{or} *H. gracile*, var. *lancastriense*.

Plumbus O. March 11th 84

My dear Prof. Watson.

Your kind letter, ^{of the 6th} received yesterday surprised me very agreeably; for I was expecting great objections against that key of the subgenera, and really, though I have searched very much, how it would possibly ^{be} better arranged, I have found nothing. There should be a (3), as well you remark, before n° 15. I write one; but see what it is! a mere cherille as we say in french; and I can say nothing else, indeed! I supposed that the change of hyp ~~x~~ instead of ~~x~~ would be sufficient; but I feel that something is not right there. The descriptions of the characters of the subgenera are right but there is no character common to all.

About *Phytidium*. The ^{sub}genus is so close to that of *Scorpidium*, that I think I united under this last denomination the two species. *H. rugosum* & *H. surpoides*. I am however not certain. And therefore read description of *Phytidium*, in case *H. rugosum* should not be described with *Scorpidium*. I have no copy of the last corrections made in the manuscript and therefore am reluctantly forced to give you the trouble to

arrange that matter, which, I know, you can do much better than myself.

I have received yesterday with your letter the paged proofs pp. 275-278. I find only a correction to be made, p. 276. 4th line from the base. If it is still time, please substitute: leaves squarrose - spreading for bases squarrose, spreading. The ~~leaves~~ are spreading-squarrose only when moist ^{but} ~~only~~ open or recurved at the apex when dry. That might be named subsquarrose? But the character is explained in the note following the diagnosis..

Do not spare me your remarks and criticisms. They are always received with gratitude. I shall be, however, very glad when the manual is finished; yourself still more! Very sincerely yours
J. Leveillé

Columbus O March 14th/84

My dear Prof. Watson.

I had forgotten the locality of *Hootkeria* *Sulivantii*, in California. It is all right. The specimens have exactly the same characters as those of Ohio. But is it a *Hootkeria*?

For *Hootkeria anomala*, Mull. (*Racomitrium anomalum*, Schwægr.) please see Mitten's remarks in Jour. Linn. Soc. XL, p. 397. with which I fully agree. That moss has not the character of Northern moss; but is evidently Austral. I don't think it should be admitted in the Manual. Nevertheless and to show that it has not been overlooked, it could be remarked upon by a short note like the one herewith.

About *Hagiothecium* *Dominianum*. I have marked it (in my first manuscript) as var. of *P. denticulatum*, on Sanderberg's etc. decision. As I have not seen that variety nor received any sketch from James. I do not know where nor on what ground Sullivant has put it with *P. puliferum* which itself is already difficult to separate from the small form of *P. denticulatum*. Please to mention where Sullivant has made a

remains about that now, and will
arrange matters when we come to the
Plagioclase.

Most thankfully and truly yours
L. Augerius,

I return proofs pp. 459 - 490.

Columbus March 15th 84

My dear Prof. Watson.

This morning, I mailed a package of proofs and a letter. I will do the same this evening sending proofs 491-512 -

Anemodon longifolius, Hartm. has never been found in America. There is a mistake, I believe in Sullivant's assertion. The specimens sent to him as A. longifolius? have been recognized by him as A. attenuatus. There might be perhaps something about that in Sullivant's herbarium, but I do not think so, as I have seen often through his specimens.

Though Omalia, fig: T v, is figured from Hypnum Wrightii, it is quite right as Omalia, that Hypnum being as Omalia by the more visible characters. But Hypnum of T. v. is not right now; the name should be Brachythecium.

The subgenera figured in Pl. 6 are, as far as I can remember, for I have not copied the names separately:

Thelia, Orthothecium, Pterogonium, Gnada. Deskea. Eurychium, Plagiorthecium, Brachythecium, Rhynchostegium, Thuidium. Tamniun, ^{Hypnum} ~~Hypnum~~ and Hylecomium.
There is nothing good for Orthothecium.

There are some other unimportant genera which are not figured, as the descriptions are sufficient without figures. And also I have nobody here who could make small reduced figures and the anatomy of any species of mosses. And therefore had to copy from Schimper. For the ^{names of the} species, at least those taken from Schimper, they can not be given, Schimper has not done it and has merely figured fragments or stems making characters of the stems and capsules of other species more distinctly showing the characters of the fructification. At least I suppose so, as he has copied Sullivant's figure of *Hypnum Wrightii* for his *Homalia* which though right for the genus can not be indicated for that *Hypnum*. Of my figures, *Thelia* is taken from *T. hirtella*. The others are from Schimper.

I see in my note, that you have numbered the genus *Leptodon* as 102. It should be 103. I suppose it has been corrected upon the proofs. Well! I think you should ^{be glad also} to see the near end of that tedious and enormous work which you have so generously undertaken and so laboriously and admirably performed. I shall also be very glad to see the end of that work;

but I have missed *Diakroton* which I do not rec'd how to correct.

Very sincerely yours,
A. S. G. V. S. S. S.

Columbus v March 21th 84

Prof. Sereno Watson Cambridge.

My dear Sir.

I have received yesterday and arranged to day the description and synonymy of *Hyppium pulchellum*, according to your notes for which I am much obliged.

About *H. pallens*, Lindb., I know only what Schimper says in Syn. Mus. Fr. p. 611 where *Prunidium pallens*, Lindb. Mst. 1864 is described in the length apparently from specimens received from the author. Schimper remarks that the species is similar to the American *H. gracile* by the inflorescence, the capsule and the form of the ovary; but differs by the stem leaves much narrower, longer and longer acuminate, not sulcate, those of the branchlets much more papulose and sharply denticulate at the apex; the pedicel of the capsule shorter.

Of *Hyppium varium* of St. Louis in Drummond? Musc. Amer. Schimper says, l.c. that it is intermediate between *H. pallens* & *H. gracile*, differing from both by the leaves nearly smooth.

Of course *H. pallens* Lindb. has nothing to do with *H. Lancasteriana*, a variety which I have long time studied, as I considered it different from *H. gracile*; the common form in Tullis. Musc. Allegh. having the leaves much longer and longer acuminate as described for *H. pallens* but not narrow. If Lindb. has acknowledged identity of *H. pallens* with *H. gracile*, it is of course useless to publish or describe it. From

what I say above, the only difference would be for *H. pallens*;
the narrower leaves, ^{thence the branchlets sharply dentat.} that form of *H. Louis* has been mentioned with
H. gracile. Could not *H. Virginianum*? Bird should be added as synonym?.

n. 124
I will mention that *Amblytelium terratum* Br. & Schp. But I
believe that it is *H. compactum* Muller. which is *H. serpens* var. *compactum*
Dumort. differing essentially from *H. radialis* by serrate leaves. I have
not seen any specimen of it.

n. 131
Hypnum Beckii Austin is unknown to me. I have not received
that N^o (1873) of the Reports of the N.Y. University. Will write to day
to have it, if possible - and will describe it or say what it is, if I
can find out. I will also describe *H. turgescens* which is a very fine
mon. and send description with that of *H. Beckii*.

H. trichosporum for *H. orthocarpum* is very acceptable -
and for all your remarks, I am sincerely grateful.

I have just now received galley of the first 10 pages of
the manuscript of *Hypnum*. But the key of the subgenera is not
with.

Very sincerely yours

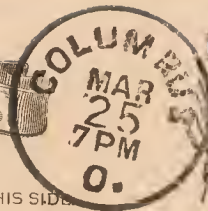
A. Desguereux

Dear Prof.

Columbus O. 25 Mars 84

In the last paged proof pp. 284 - 298, I should
not the heading of p. 286 be Meteorium, instead
of Homalia. - P. 288 line 20, after chestnut-color, instead
of: is not; right? - I have found nothing else

Very truly yours
A. Vergara.



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE

Prof. Sereno Watson
Botanic Garden

Cambridge
Mass.

Columbus March 27th 1844

My dear Prof. Watson.

n.135 I must thank you to the end and
never so much as I should like. Now it is
for Leptodon Floridanus of which I will take
good care for additions. Hypnum Virginianum
has been mentioned in the returned galley and
from a note received of Prof. Beck, H. Beckii;
Austin is a mere sterile form of H. ~~Virginianum~~,
this, conceded by Austin. Mr Beck has the
kindness to send me a specimen from which
I can see or rather so far as I can see is referred
to the variety Subulaceum. I will mark it
in reaching the proofs. As for my fortunate escape
of those 29 new species of Mueller from Beckley, Wright
we might take them on hand in the additions.
But as you are doing most of the work now, the
escape is more to your advantage. Indeed I do
not know how I could have gone along without your
assistance. And just now, my health is more
precarious and every thing of work more difficult.

As there are a few corrections to the passed proofs,
I return the proofs for your inspection.

Very sincerely yours A. Lesqueres over

I now receive proofs (galley.) p. 531-548 and
will try to read them for returning them to morrow.
But I have not left my bed for five days and
my poor head turns like a windmill. Still so
the best. And indeed if I should stop worth all together,
it would not be much loss as yourself
are doing all the work. - By the same mail
I have the obituary notice of Dr. Engelmann.
As I am the oldest of the lot, that is encouraging
or promising. I do not understand how
the notice has been delayed for more than one
month. Dear Engelmann. I have seen him
only once at St. Louis, but to see him was to love
him.

Columbus V April 3rd 84.

My dear Prof. Warton

I return to night the galley of p. 571 - 647
received this morning. That *H. denticulatum* var
Donianum, considered by Sullivant as the true *Pla-*
gothecium piliferum (Pch. & Schz.) would be put
as syn. as I have marked it in pencil. But
as you do not mention the N^o of that Drummond
moss, and as I find no record of a Drum^d moss in
the var. *obtusifolium* of *H. denticulatum*, ^{nor of any locality in the Rocky Mts.} I think
it is quite as well to leave out the remark
of Sullivant. - Any way you chose, I shall
be pleased as I know you will take the best one
Very truly yours
A. Senguer

Columbus 012th 84

Prof. Sereno Watson.

My dear Sir.

The proofs came yesterday morning and were returned to mail in the afternoon, but yours of the 8th was received only in the evening. Some times some of my letters are delivered to my son's store and they bring them to me in the evening only after business time.

That *Hypnum oxycladon* has never been mentioned to me by Sullivan; at least I have no note about it and of course no recollection of it. The species is a *Campylothecium*, as far as I can see by the description and quite near *C. nutans* Schreb.; perhaps merely that variety found only sterile, which is described in the manual. I think it would be best to include it in the addenda. It is very regrettable that we can not have a specimen.

The glossary can be ready at any time and may be placed as the editor thinks ~~more~~ advisable. Please let me know when to send preface and glossary. For the rest somewhat better and up most of the time, I can write very little. — You have taken from my shoulders the big load of the work about the book and the trouble also, in such a ~~note~~ ^{way} ~~method~~ ^{and is} that thanks to you all is going ~~surely~~ ^{surely} far better than

both James and myself could ~~never~~ have done
it ~~seemingly~~ even with the assistance and direction of
Schimper. Hence I can but consider the authorship
of the work as legitimately due to you as to me.

Very sincerely yours

L. Desguery

My dear Sir,

Columbus 015th April 84

There are in the last proof (revise) a few errors which it may be worth correcting. But perhaps it is too late or you have seen them already

p. 299 Counting from above line 24 asperella, should be asprella
307 " " 7 Mount Diablo " Diabolo??
307 " " 9 Margins " margins
315 " " 15 as long as or longer. " erase as

- Very truly yours

Leigueren



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE

Prof. Sereno Watson
Botanic Garden
Cambridge Mass

Columbus O April 17th 84
Prof Lewis Watson. Cambridge.

My dear Sir.

I received yesterday or night your kind note of the 14th. As the Glossary was ready, I send it to day with a kind of preface which I have rearranged as best as I can. You will, I know, have the kindness to read the glossary and make to it any correction you may think proper. For the Preface, I do not know what to say. It is perhaps too long; but I do not know how to shorten it except by erasing the whole. I am delighted with what you say of Prof Gray and Mr James assenting to your co-partnership or authorship. You can not refuse that. If you have not used the microscope for the examination of money, you have studied them in the books much better than any author, except Lundberg; much better than Schimper, especially. You have collected the most rare species of the Pacific. You have indeed by your assistance in the publication of the manual made and merited more than James and myself. I sincerely demand of you this new favor which I would consider

the greatest service you could ever render to me.
I am under such great obligations to you that
if you will not take your right as author with
James and myself, I can never get rid of a painful
sense of indebtedness that I am unable to repay
and that has constantly troubled me since you
took the book under control. I can do not
refuse this last service to me and to the end
of my life, I shall remain your friend.

I will now give the addition of species.
Beside those you name, I have in my memorandum
Hypnum fargesii from Davis straits.

H " *Vaucheri* collected by Macoun in Canada.

fide Austin or rather Raw

H " *planum*, common in Florida.

H " *alpestre*, Id. Id. Am. Bot. fide Austin.

I shall I mention some new localities which
seem remarkable enough.

p. 21 21. *Sphagnum Austini*, sent to Raw from Florida.

Bartramidula Wilsoni has been noted from N. Jersey
to Austin.

Philonotis tenella also by Austin

Anacanthium Hornschuchianum N.W. Coast Austin

Dicranum Bartramboni Hook.

I think I have mentioned these two last
species; but part of my copy of the paged proofs
has been lost or stolen and I shall only be able
to see about that when I have the copy
which you promise me of the whole volume.

Stam has also sent me a specimen of *Sphagnum*
aribosum Lindb. But I do not find it described
in Brathwaite and have not Lindb. *sphagnaceae*
is that species described there?

But I must hurry to other work now as the
package of proofs is just received; but there are
not yours or those you speak of in your letter.
I have not seen them yet.

Sincerely yours,

I. S. Squire.

Columbus, O April 19th 84
Prof Sereno Watson Cambridge.
My dear Sir.

I have received to day and read
paged proofs 351-373. Few corrections to
be made but some remarks which are
perhaps of no moment.

p. 351. line 8 from base after pseudo monoxium, for:

* " " 9 " narrowly-rhomboidal, erase -/

p. 354. line 8 from base. for tendrils you substitute branch
lets. I think stolons is right.

p. 367. line 9 from base Donmianum for Doncanum.

p. 368 line 13 from above. The locality. Adirondacs M^{ts},
chs H. Fish could be added; there is place in the line.

p. 369 line 14 from above orthocladium for Orthocladium

* p. 373. line 6 from base Bryol. Eur. 505 should be 565.

~~Would~~ it not be advisable to add after this
page. the note in Sullivan's Moons of the U. S.

p. 78 after H. radiale, about that so called new
species of Schimper mentioned in Bryol. Eur.

[Amblystegium p. 11.] as Amblystegium verrucosum.

The note of Sullivan would be quite right
and say as much as Schimper does on the
characters of the species.

And also, would it not be right to quote
~~Sheep~~ *H. graminicolor* (Bridg?) Wils. & Hook.
in Drum. S. mon. N° 133. as synonym
of *H. sullivanii*, which according to Muller
has been found in Penn. by Muhlenberg.
The ? after Bridg. renders the synonymy un-
certain. But Sullivan has quoted it in
that way in Mon. of U. S. p. 64.

If you find it right, that could be mentioned
in the Additions and Corrections, end of
the volume.

I have written already the description of
Bartramichla (genus) and *B. Wilsoni*,
Philonthus tenella, two species in R. &
Hervy Catalogue put there on the authority
of Austin. as found the first in New Jersey
But the last, *P. tenella* is not needed
as it is described as ^{variety} of *P. Muhlenbergii*,
p. 218. of the Manual.

Then *Leptodon floridanus* Lindb. *Hypnum*
oxyladon Bridg. *Hypnum Vaucherii* Schp.
found sterile in Canada by Macoun, according
to Austin. *H. alpestre*, Swartz also said
to have been found in N. America (Austin)

Hypnum turgescens Schp. Davis & Grant.
and *H. planum* Schwæg. Common in
Florida.

I may send these pages now if you like
But as you will send me a copy of the
whole descriptive part for ^{making or correcting} Index, I may
in reading it again, find some more addi-
tions or remarks to be made.

If I shall make the Index, please direct
me how to proceed for names in Roman
or Italic. Is not that of Sullivan in
Mon. of the U. S. well arranged. I would
follow it?

Very truly yours
L. Lesquereux

Columbus 23^d April 84

My dear Prof. Watson.

I return to day the galley pp. 651-674 end of the descriptive part. I find nothing to correct.

Herewith you will find the few additions and corrections that I have noted. Three of the species: *Hymen alpeke*, *H. fuscus* & *H. planum* may be added in their normal place to the Galley; as you will see, their place being marked at the heading of the pages. I suppose there may be some others. In any case, I am ready to follow instructions, if you will have the kindness to give them.

If I have to make the ^{Index} ~~plates~~, please to say if I shall follow Sullivan's for arrangement & type. There may be a few words more to add to the glossary. But if I should have left out of it all the words described in Webster, very few would have been left in it.

I will read to night page proof 374-388 and write if I find some errors. Remark that the last page 388 is written 338.

Very truly yours
A. Dequevau

My dear Sir.

Columbus April 23rd 84

I find in Bot. Gaz. p. 111 line 6 from above the following.
" There is another sterile (always?) Bryum growing in great
profusion on the White Mountain associated with the
preceding (Bryum Braueri) and with Leptotrichum
homomallum. It resembles Melichioseria nuda in
most respects, but is larger with the stems often flagelli-
ferous. It occurs also in New York & New Jersey etc.
Is it worth to take notice of such a remark and
have a species made of that? Please see about it,
and oblige yours very truly

S. Dequereux

1. 372 l. 12 from top upwards & erase &

" l. 13 " segments 5 let 5.

379 l. 11 from base erase at base or below

388 is printed 338. I find nothing more



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Prof. Henry Watson

Botanic Garden

Cambridge

Mass.

(Plumbers 0 April 26th 84

My dear Mr Watson.

I return herewith the Introduction with scarcely any change. of course, if there is anything not acceptable to you, you will please set it right just as you think proper. I have regretted all the time that I did not abandon the work, but now it is too late. That should have been done after its first examination by Prof Gray and also, I think by yourself.

Mrs James said, I think that I should be entitled to my share of the volumes given to the authors, 25 to each, if I do not mistake. You would pay me a service to direct ~~the~~ editor to send me only 12 copies and deliver the others & you or to Prof Gray for distribution according to your liking. I owe three or four copies in Europe, one to Lindberg, one to Renault, one to Schimper the fourth has not the editor of the *Revue Bryologique* who will give a favourable mention of the manual. Renault writes me that the book will be and is already in demand in France and that he would like to know if the book will not be put for

sale at some french book store; that should
be done at Paris. I will also, if I receive my
copies send one to Carl Mueller and ask him for
some mention in a Scientific German Paper.
Schimper can do that also. He is now at Bonn
returned of his travels. In the country, I owe
a copy to Prof. A. Braun. and perhaps to Prof. Vortel.
I will send one to Bolander. I mention this in order
that if yourself or Prof. Gray distribute any, two
copies should not be sent to the same. Prof. Gray
will know if Prof. J. D. Dana or Prof. Salter should
accept one. I may also send a short notice to
some scientific paper - and perhaps I should send
also a copy to Coulter Gray and to the Torre Club.
Very sincerely yours

A. S. Senguer

Perhaps, for Distribution in Europe, the best would
be to send some copies to Mrs. James with a
list of addresses ~~for~~

Columbus O April 26th 84

Prof Henry Watson Cambridge.

My dear Sir.

I return this morning the Explanation of the plates received yesterday. I thought at first that what you said of the figures taken from Schimper did relate to the Explanations of the plates or rather to the plates of Schimper's Synopsis which Schimper prepared in imitation to those of Sullivan; but that relates to Ernst Schimper, Org. Eur., which Sullivan has quoted in the Explanation of Mones of United States. Therefore the whole may be left as you have written it. The cilia added to the peristome of *Pseudobolus* are those of *P. catenulata*. I thought best to show the peristome (cilia) of this last species, as that of *H. atro-*
viridis is not beyond, ^{but of a Lophop} The cilia is represented by figure copied of the same. It is therefore of Sullivan not of me.

I have just received your kind letter of the 24th and the Introduction (should not that be a Preface?) and I feel really greatly troubled with what you say of your work and with the little part you take for yourself who should really have the best. I can not take upon myself a load of

indebtedness which I do not see the means of repaying in any way. I feel that with your precise mind & with the exactness of your views, you can not take part of the responsibility of a work which, as you say with right, should have necessitated a more thorough revision than that which you have made. And therefore I say that you do well not to accept that responsibility. But it is unjust to refuse the demonstration of all what you have done for the Manual. - I can not write now. My head is in confusion. I will perhaps say more after noon in returning the Preface.

The first title suggested was: Synopsis of the fauna of the North American continent. Either yourself or Prof. Gray remarked that the word manual was best. But as the work is done now with addition of a synonymy much more perfect than that of Schimper or Muller and as the descriptions are somewhat long and detailed, it seems to me that the word Synopsis should rather be accepted or acceptable. For this as for all other matters, I shall gladly submit my opinion to you. Very truly yours
V. Arizpe

Columbus O 28th April 84

Dear Prof Gray

Will you pardon me to come to you again and call for your assistance once more about the work on American money which is all ready now. I sent a kind of preface to Prof Watson, modified from the first one made before his assistance in preparing the Manual. Prof Watson will have nothing to do with authorship of the book. I well understand his motive though that refusal pains me. We have exchanged letters on the subject. That preface sent to him does not suit him either. He says that I am making too much about his trouble, hard work and time lost. Now Saturday I returned to him a preface of mostly revised of the one he had modified and sent to me for approval, and I merely changed the part concerning him which I find not at all satisfactory in regard to what he says of the little done for the work. Would you not please have the kindness to see both copies of the prefaces prepared by myself and consider what I had best to say in order to satisfy M. Watson and

my conscience also. I can not accept without
any kind of remuneration, the time and
work given by Mr. Watson to the manual of
the money, and though I think and hope
to be able soon to offer him a compensation
in money, I wish the biologists to know that
part of the work is not of me and that
Prof. Watson has taken charge of the revision
against great loss of time and much trouble etc.

When the Manual or Synopsis of the North
Money of North America (I have not yet
fixed the title; could you not help to do it),
is ready, I will still take the liberty to
write you again and express my obligations
to you; for you have encouraged, fostered
and brought out the publication of the book.

Very sincerely and respectfully
yours old friend
J. Serres

Columbus April 30th 84

My dear Prof. Weston.

As soon as I got your letter, yesterday, I wrote to Prof. Gray. But to day, I think it would be advisable to show him the proposed preface which you returned to me and which, I believe, he would approve rather than yours. Will you have the kindness to show it to him - I am sure he will find the matter satisfactory.

Very truly yours

L. Sengueray

Stumbers O 2 May 84

My dear Prof Watson

As there are some errors, I return the proof of pp. 389-411. - I do not know where I have seen circinnate with two n, and I can not find. It is probably one of my lapsus memoriae. Your addition to species of uncertain affinity is quite right. That locality, which was called Summit or Summit portage before the building of the R. R. crossing the Allegheny from Altoona should be named with a $\frac{1}{2}$ Summit or Summitville, as it is now marked upon the Gazetteer. But perhaps Summit Portage is best, as the village Summitville was not in existence when I collected moose on these mountains. - I am glad those species could be described at their place. There is scarcely any addition to make.

I got the proofs yesterday evening. Perhaps it would be best to return them to me if I have to read the whole work through. But my series of proofs is incomplete. I return a parcel you had the kindness to send me before.

Facutate is not described in the Glossary. Please to put it in.

Very truly yours
J. Leizureux.

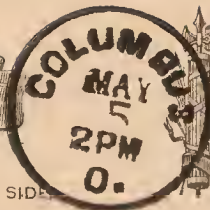
Columbus O May 5th 84

My dear Sir.

Glad you have found that. I could have seen it only in preparing an Index. I think *Hexapetalum* would do. It is according to description or descriptive of the species. *Semipetalum* would do but less appropriate.

Very truly yours

L. S. Greeney



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE

Prof. Sereno Watson
Botanic Garden

Cambridge

Mass

Columbus O May 6th 84

My dear Prof Watson.

I return the proof of additions and explanations of the plates. - That H. attenuatum, Brd. has all the characters of H. latum and could not be mistaken for it, if it was not that this last species has a dioecious inflorescence. The characters are not all described; the costa, the areolation; the alar only is given. It is therefore difficult to say what it may be except that it is not a Eurynchium. - There is still another blunder caused by my trusting implicitly to Schimper as I have always done to my great regret. ^{now} H. murale is not a Eurynchium but a Rhynchoetogium. As you remark that the plate VI is entirely made up from Schimper, this case he left unnoticed for the character of both so-called genera are the same for plain sight or without microscopical examination, or even with this. But the figure is like that of Eurynchium crassinervium, Taylor and if you think best, this name might be substituted to that of H. murale.

Very sincerely yours

L. Legermann

Columbus O. May 8th 84

My dear Prof Watson.

Will that do? Please give me
your assistance to the end and correct, if
the title does not seem right to you.
Manual would do as well as my opinion.

Very truly yours

J. D. Gregory

The sentences between () may be omitted.

Synopsis
of the
Mosses
of North America
(including New Mexico and Greenland.)
by Léo Lesquereux & Thos. P. James.

with six copper plates.
illustrating ~~the~~ Genera.
(and subgenera of Hypnum)

Columbus O May 14th 84

My dear Prof. Wabor.

I return the proof of the preface which I find appropriate. I am much obliged to Dr. Gray for the modification; only I should have liked them still more, if he could have said something of my obligations to him as he gave me the first direction concerning the work etc. and exposed the necessity of a revision of the whole work. I shall write him again when the work is out. For my obligations to yourself, be sure that I do not find them canceled by what is said in the preface of your cooperation to the work and though you may say on the matter, I shall not rest content until I have given to you an evident proof of my gratitude. I well understand that you worked in remembrance of James. But I never the less reap the profit of your work.

When you know something about the distribution of the volumes given by the publisher, you would greatly oblige me to say to whom I may distribute the few copies I have to get. As I wrote it to you some time ago, I should like that half of my share was given to yourself and Prof. Gray.

Very sincerely yours
J. Denker

Columbus O June 14th 84.

My dear Prof Gray.

The manual has come yesterday (25 copies, and really I have seen the book with great pleasure. It looks well, attractive, and will be of some advantage for the study of bryology. I wish I had had such a book to study the mosses. — You will be surprised, I think, that I dare to take the liberty to send you a copy. I well know that you may have one dozen or more from Mr. Garner or Mr. Wotton; but you must have the kindness to accept one from me and to keep it in your own library in remembrance of your most charitable assistance in the preparation of the work. I owe to you not only the critical remarks and the valuable directions received before the beginning of the printing; but the book owes to you also all the work made by Mr. Wotton who could not have been induced to give his time to such a work without your instance. And for all that you did not even allow me to acknowledge my services to you in the preface. I know that you have done a great deal

of work of that kind, in assisting others without accepting any reward. But it is no reason why I should accept your kind offices and say nothing about them. I can do nothing more however but ~~to~~ thank you most heartily now.

To Mr. Watson, I owe still a debt that I can not fully pay but that I will nevertheless try to partly cancel at least to quiet my conscience. I know that he will not accept any money from me. But, as I have done only part of the work for which I have been paid, I intend to return to Mr. James \$100.00 as belonging to Mr. Watson. I beg you will not mention the matter to him. But really I can not accept money which I feel I have not earned in fulfilling a contract partly completed by the work of another.

I suppose that I have to tell you now good by for a long time. I have soon completed all the publications which I had on hand and feel that my work is soon ready for the world. I shall however sleep, as long

as I live a deep remembrance for all the acts of kindness and goodwill received from you since ~~my~~ I arrived in America and a sincere affection.

Please to remember me kindly to Mr. Gray.

Very respectfully your old friend
S. Loring

Mullen
Lindberg
Schimper, J. E.
Henslow
Renard

Braithwaite
Mitten
Rept
Maximowicz
Macoun
DeCandolle

Ram
Smith
Bolander
Gray

Columbus O. 14th June 84

My dear Prof. Watson.

Yesterday evening, I received 25 copies of the manual of the Mosses of North America, sent by Messrs Cassino & Co. I am very much pleased with the book which looks really nice and attractive. About its intrinsic value, you know it just as well as myself. Mr Cassino says that he sent you also some copies. Don't you like to have any more. I would gladly send you half my lot. Send a copy to C. Mullen, one to Lindberg, some to Franc. I never corresponded with Mr Mitten, nor with Mr Braithwaite and suppose that Mrs. James will distribute some of her copies in England. But if you have some acquaintances or if you know of some persons to whom you think ~~a~~ copies should be sent; please let me know and will immediately attend to the matter. Of course I will send copies to those which have contributed some material to the book. Ram, Dornell

Smith, etc.

What now gives me some anxiety, is the matter on which I have written to you ^{often} already. The more I consider the time you have given to the publication of the manual and the trouble the work has given to you, the more I feel what heavy task and loss also the work has imposed upon you. But I know already what you will say and I do not ask you to answer me on the subject. I write this only to let you know that I shall not forget, as long as I live, the great service you have rendered to me and to biology, if even I am never able to acknowledge your kindness otherwise than by mere words.

I should now like to write to Mrs. James. Do you know if she is soon to come back to America or could you not give me her address.

I will also write to Prof. Gray and send him a copy of the manual. Of course he has got one from yourself or from the publisher, but I wish to offer him one myself. He has had a great deal of trouble for the work too and it is to his recommendation that

I owe your assistance.

I suppose that you are now quite pleased to be out of the job. (as the Americans of this country say), and that you enjoy the advantage of being able to quietly pursue your labor. For myself, I have often and deeply regretted to have accepted the duty of that publication. But now that it is done, I believe that ^{the book} it will be good for something and acceptable to some.

Most sincerely yours
L. Lesquereux

Columbus O June 26th 84.

My dear Prof Watson.

You will have to excuse me once more, for the letter, first, and perhaps for trouble of a new kind. I have sent to those who were interested in the preparation of the manual of the Mones, and those who could make the work known, 15 copies of the book. I have now 10 left and beg you will accept half of the lot. Perhaps you may later have occasion to present them to some friend of yours or to exchange for some books. I sent a copy to Muller, Lindberg, Schimper son, Hesnot, redacteur of the *Revue Biologique* of France, Renaud; and in the U. S. to those who have contributed most to the world by communication of specimens: Baur, J. Donnell Smith^{Botanist}, etc. The road is now clear for the future progress of the work and the copies left on hand may be preserved for casual demands.

As I am not in position to receive and read any scientific journals, I hope that if you find somewhere critical remarks

on the Mammal, you will have the kindness
to let me know about them. The work
will certainly be valuable to Bryology.

I do not like to take leave of you with
whom I have had for months such pleasant
intercourse and hope to find reasons to
authorize me to visit you again. In
any case, I shall remain henceforth a
sincere friend of yours, this without claiming
any return; for I am the obliged, greatly
obliged indeed, while you have had only
trouble and hard work caused by your
relations with me.

Very sincerely yours
S. Augereau

Columbus O. Sept. 17/84

Prof. Lorenzo Watson Cambridge.
My dear friend Sir,

I return to day with best thanks the
N^o. of the Nation where I read the notice of
Prof Gray on the Manual of the mosses. The
notice is very kind and good. I think, however,
that Prof Gray is mistaken in supposing
that Bryologists of the next generation may
come to reduce the number of the genera and
orders and thus increase their weight and value.
The tendency is the contrary way. That, you may
see by Mitten, Lindberg, Braithwaite etc. The influ-
ence of the mosses is of the same character for all
except perhaps for the Sphagnaceae. They have the
anther, the archegonium differently distributed in the
same species, or similarly in a large number of
different species, genera, sections, etc. The capsule
with its modifications in the peristome does not
offer any persistent character or and difference
which might be relied upon as essential character
even of species; hence, the Bryologist has to
rely for his subdivisions to groups of forms,

the ramification, the leaves, the areolation etc. which are differently appreciated by every bryologist and have therefore little strength for the basis or foundation of large divisions. After a long and tedious study even of a single order of the mosses, the Bryologist is forced to acknowledge that all divisions and subdivisions are an affair of opinion. That a group for example may be subdivided into such and such species or not divided at all. And that therefore, if we should admit for a genus, groups of species related by one side only, like *Pistillaria* and *Dicranum*, we have no reason to separate them like *Bryum*, *Mnium*, *Aulacomnium*, *Bartramia* etc etc. and therefore give to genera such a wide extent that the number of ^{we must} species becomes more troublesome for determination than are the great number of genera whose character can not be definitely circumscribed. I should have much to say on the subject and may prepare a short paper on ^{it} the subject if I can find not the time, I have plenty of it now, but the mind clear enough for the discussion.

I hope that you have had a good time of rest during the vacation and perhaps have had an opportunity for visiting and exploring some countries of the Western Region.

Have you in your own library the *Histoire des plantes* of Baillon? I have it beautifully bound except the last published part, the *Melastomaceae*, *Cornaceae* and *Umbelliferae*, not yet bound as it is only ^{the} half of ^{the} *prothum*. If you do not have the work and if it could be acceptable to you in the least, you would confer to me a true favor in accepting it from me as a kind of remembrance of the hard times you have had with me and for me. As I am a subscriber, I would continue to receive the parts as soon as published, (they come very slowly) and send them to you. I would also be much obliged to you for advising me of the return of Mr. James to Cambridge.

Very sincerely and respectfully your
old friend
J. Sarguerum.

Columbus O Oct^r. 1st 84

Prof J. Watson Cambridge

My dear Prof.

You have, probably seen in Bot. Gaz. last number the criticism of Mr. I. S. Rawn. I had so much to do that I could not look over the matter until today.

According to Mr. Mohr's statements in letters, he *H. hosnaltlegium* and *Trichostomum macr-
legium* are species which have been found in Mexico by himself and which have been casually mixed with his specimens of *mobile* in the transfer of his herbarium. But *H. thetislegium* Mull & *H. occidentale* Tullis have been forgotten and I cannot in any way account for the blunder. For the last species only I find that the last plate 81 of the 2^d. vol. of the Icones is adhering to 80 this duly marked as recorded, the other not. But that is a poor excuse. About *Dicranella Canadensis*, I think that the species was published by Witter after I had named the *Dicranum*. But that is also inexcusable as I could or should have described the species at the end of the volume in the addenda -

Now, please tell me what you think I should do, either send to the Gazette a note acknowledging the omission and giving a short description of those three species or let the matter drop.

Please excuse me. I am sorry to give you the trouble to come again to this subject, the Manual, which has troubled you already too much.

I have a letter from a Botanist of Hungary Dr. Bernier who sends me a new species of *Tranygl-^{moss}* and another from Prof. E. Bescherelle of Paris who says that the librarian M. Berquer who received copies of the Manual for sale from M^r. Cassino has already none left. I wrote to M. Cassino about that.

Very truly yours

L. Serquere

Columbus O. Nov^r 19th 84.

Prof. J. Watson, Cambridge.

My dear Sir.

As far as I have had the opportunity to judge, it seems that the Manual of the mosses has been generally well received. Lately, I have received from Prof. E. Bescherelle of Paris a very kind note on the book, together with all the memoirs which he has published on mosses, as follow:

- 1 Notes Bryologiques. - 1865. Bull. Soc. bot. de France
- 2 Mousses de St. Paul et d'Amsterdam —
- 3 Structure des tiges des Bryaceae. 1868. Bull. Soc. bot. de Fr.
- 4 Notes sur les mousses du Paraguay —
- 5 Notes sur 3 espèces de mousses de la Nouvelle Calédonie 4
- 6 ^{Florule} Bulletin bryologique des Antilles Françaises. Ann. Sci. nat. 1876.
7. Notes sur les mousses des Colonies Françaises. Bull. Soc. Bot. 1881.
- 8 Florule Bryologique de l'île de Nossi-Bé. Bull. Bryol. 1880.
- 9 Florule Bryol. de Maurice & autres îles. Ann. Sc. nat. 1881.
- 10 Catalogue des mousses d'Algérie. 1882.
- 11 Florule Bryologique de la Nouvelle Calédonie.

I quote the titles of the memoirs in order that you may see if you have them or wish to have any of them, as Prof. Bescherelle says that he should be much pleased to lend them to you in case you would like

to have them. - He has also noticed the Manual
in some Botanical Journal of Paris and will send
copies as soon as printed.

You have perhaps read the very fine note
published by Dr. M. P. Cooke of London in the
last vol. of the Grevillea. It was sent me by
a friend. If it was not too complimentary, or if I
was not interested in the matter, I would consider
it as the best notice possibly given of the Manual.

Now, we have received, it seems about all what
has been published in favor of or against the Manual
and apparently the book is really a useful one
or will be so hereafter. Thanks to you for that,
my dear Sir, for I feel more deeply every day how
poor that publication should have been without your
cooperation.

I have not yet written to Mrs. Janier, but
will do it to day.

Very sincerely yours
Léon Desquereux.

My dear Sir. Columbus O. May 18 / 85.

I owe to your kindness the communication of
a number of the Botanische Zeitung (which I return),
where I have read with great interest and pleasure, your
article on the Manual of the Mosses of North America.
I thank you sincerely for it. It is all what I should
have wished to say if I had read the critical remarks
of the Zeitung. I should like to write more on
the matter and also about mosses, if I thought that you
are now at Cambridge. But I have read in a paper that
you had gone to South or Central America for collecting.
I hope that your journey will be most pleasant to you, as I
know that it will be most valuable for Botany.

Very sincerely yours
Leif Sengueren



COLUMBUS
MAY
18
27th
1885
U.



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Prof. Lorenzo Watson
Botanic Garden
Cambridge
Mass.

Columbus 10 June 7th 85

My dear Prof Watson

Your counter-critical remarks in the Bot. Zeit. are not merely *à propos*, but right and most satisfactory, I am sure, to all the Americans students of mosses. I have thanked you heartily for them for as far as I am concerned, as well you know, and nobody was more interested in the matter than myself.

I am very sorry indeed to know that you have returned sick of what apparently was a perilous exploration. Science will profit of it certainly but I am nevertheless very sorry that you have to suffer now for your researches. I desire very much that you may be soon fully restored.

You will be surprised perhaps to know that I have sold my herbarium of mosses to the Museum of Neuchâtel my native town. There were three parties interested to the sale, one the University of Bonn, the other the U. of Lille France. Though my herbarium can not be of great utility at Neuchâtel, I had to give the preference to an

institution to which I owe my education of
youth and which was my alma mater. You
have for the masses of the U. S. Sullivan's and James'
collection both more valuable than my own which is
extremely rich only in European specimens and also in
endemic species. — Indeed, if I can do it, I will try
to write an introduction to the study of the mosses, for
I regretted much that such an introduction could not be
admitted in the manual. You know I wrote something
about that matter. But it was returned to me by Prof
Gray. — I should be much pleased if I was able
to go on Eastward to Cambridge and pay you a visit.
When my money is paid, I shall be in position to
offer you some compensation for the time you have lost
in working for me. Please say nothing about that. I
will know that you will say no. But I ask that for
my own satisfaction. I owe you much; and though the
indebtedness is not heavy or troublesome to me, I would
be more satisfied, more pleased with myself if I had
in some way shown to you my gratefulness for the
work you have done for me, a work whose usefulness
is every day more evident to me. If my health keeps as good
as it is now, I may decide to pay you a visit in some month
very much and respectfully your friend A. S. Longueur

When money is paid to me have will be paid and paid

Sittoum 12 July 22nd 85

Prof Watson Cambridge.

My dear Prof.

I have been here about two months,
my health has been fully restored and quite
well as I am now, I shall try to go to
Boston next week and hope that I shall
have the pleasure to see you. At least I
shall try very hard. You would therefore
oblige me to let me know where I may
find you about the middle of next week.
and if you are not gone to the Polar regions
for the hot, now very hot term, I shall
go after you, if possible. For I hope that
your health is now if not entirely, restored
at least satisfactory^{now}. Have the kindness
to address line to my care of H. E. Dwyer
92 Butler St. New York

Yours sincerely yours

Geo. L. Dwyer

The two I have copied. I find nothing related to that. The stipules of *Crataegus* are double; those of *P. tomentosa* may be compared to the figged ones but they are pedicellate. If, when looking through some of your plants in the herbarium, you find something more appropriate for comparison oblig me to let me know.

Columbus 10 Sept. 21st 85

My dear Prof. Watson.

Your very kind letter of the 17th Inst. was unexpected and received with the greatest pleasure. For about three weeks, I have been looking for a reason or an excuse for writing to you, as I am always afraid of intruding and taking your time without good cause. Be sure that I shall always keep a pleasant remembrance of our copartnership in working mosses; that I like the dear mosses the more because they have afforded to me the advantage of becoming more intimately acquainted with you; and that, instead of you being now my debtor, you have added to my deep sense of gratitude, in accepting - that token of it, which I hope will remain acceptable to you for very many years to come.

I am much obliged for the commendation of that number of the Journal of the Linnean Society. It is very interesting, 1st for the exposition, by M. Miller of the Genus *Fissidens*, as represented in Europe and America. and 2^d for remarks on a *Lycopodium* by Rob. Hirston. That remark may also interest you on account of the *Lycopodium* of the Coal. - That *Fissidens Hallianus*, Salt-Lair

is the beautiful *Conometrum Hallianum*, a genus which I am not yet disposed to unite to *Fernandez* as Mr. Watten does. Perhaps I should have given another name to *Fernandez Hallii*, but this was baptised by Curtis long before after the publication of *C. Hallianum*.

We have 30 miles N. E. of Columbus or 1¹/₂ hours of N. Reading a splendid small lake, reservoir of Canal, which, in some parts near its borders is covered with *Nelumbium luteum*. The view of such a prairie of large flowers and splendid leaves is admirable and generally I go there a few times in summer to enjoy the sight. The stalks of the flowers and leaves have a peculiar conformation and constitution. Formed of five or six small canals around a central larger one, the parietes are filled with spiral vessels which, in breaking the stems, uncoil like threads of silk or arachnoid filaments of various length, 3-4 feet and are nearly as strong as the threads of the cocoons of silk worms. As you may perhaps wish to examine the matter, I send you, upon a spoon, the threads obtained in breaking a single stalk in pieces of about 3 inches long & together with a loose bundle of others derived from another stalk, where you may see better the character of the threads. The canal or

the inter parietal vessels are filled with a milky fluid especially obtainable before the ripening of the plants or the inflorescence. This fluid soon becomes hardened under atmospheric influence and has the compactness of gutta-serena. It seems insoluble in spirits. I have got a few drops in a small bottle which you can break if you wish to examine its nature.

Very sincerely yours

Le^o Lesquerens

A friend of mine, good Bryologist, M. l'abbé Bouley professeur à l'Université de Lille, writes me, asking how he could obtain seeds either exotic or American for a Botanical garden which he is anxious to continue, leaving now its direction. Do you think that I could get something valuable from the Agricultural Dept. of Washington? The climate of Lille is, about that of Cambridge, I believe

More still! Here is why I wanted to write you weeks ago, but my poor head becomes more and more out of order - I send herewith a sketch of those stipules of *Cretaceous* species, of which I spoke to you when at Cambridge. There are a few of them but all of the same forms as

Lumbus O 2^d Nov^r 85

My dear Prof. Watson.

Your kind letter of the 31st has been desired for a long time. I was afraid you had forgotten me entirely. But as it brings to me kind words and a splendid representation of yourself, I feel doubly pleased now and offer you sincere thanks for letter and photograph. The representation of yourself is very fine, beautiful indeed; but no better than the original. I shall value it very much and it will go immediately into my album of scientific friends when with Gray, Torrey and other American botanists. He will find the two DeCandolle, the oldest, Boissier, alas! now dead, Duby, and a large number of other friends though not botanists, like Agassiz and Geyser. In Europe the receiver of a photograph is authorized to send his own in exchange, and this I do now, though I do not know if it might be acceptable or even polite to do so. You will excuse me any how, I know.

An objection against the reference of those stipular appendages to Polygonum is the hard starchy substance of these fossil organisms. I should however be pleased to see one of *Pot. perfoliatum*, as I have none in my small herbarium mostly composed of ligneous plants. For consistence and hardness, those of *Coccoloba* would be quite acceptable for comparison. But the form and position are far different. Long time ago, that is about one year, I sent to Saport. the same sketches as I sent to you. But he could not find anything for comparison, merely remarking that perhaps somewhat like it could be found in the *Araliaceae*. Please do not trouble you about.

You said nothing on the possibility of obtaining seeds of plants, flowers or trees from the Botanic Garden of Cambridge. Perhaps you could give to me an address and recommendation for the Agricultural department of Washington. These seeds are for the Botanic Garden of Lille, France, an institution which perhaps would offer something in exchange.

Very sincerely yours
Léo Lesquerem,

Dear Prof.

Columbus O Nov^r 24th 85

I have opportunity to writ to Prof Beschardles
very soon and will mention what you say of his publications

I sent to Prof Poculter in Sept^r, just after
receiving your note on the subject, a short article
for the Gazett. He answered that he would be
very glad to publish it. But he has not done
it. I wrote him after receiving the last Nov^r of
the gazette, to add to my note the date of the
communication, if ever he wanted to publish it.

Sincerely yours,
L. J. Desguereux



NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE

Prof. Sewall Watson
Harvard Herbarium
Cambridge
Mass

(Thumbar 0 24th March 86.

My dear Prof Watson.

You think perhaps that I am already dead, buried perhaps? Not yet. Always hard at work with little profit either for myself or for others. I am at a new volume of Cretaceous plants, for which I have splendid materials, fine plates already made but genera and species mostly undescribed as yet. I have now from that formation plenty of leaves of the species which I have considered as a conifer and named *Phyllocladus subintegrifolius*, Cret. Pl. - Heer ^{who} has from Greenland specimens somewhat different from ours, put this species in the genus *Thierfeldia* of Ell. a genus of uncertain affinity, which, as Conifer for Ellinghouse or Gled for Schenk, is a Fern for Braun, Schimper and others. My *Phyllocladus subintegrifolius* which may be or not the *Thierfeldia* of Heer is certainly not a Fern and for me certainly a *Phyllocladus*. But for my opinion, I have of course to give reasons.

Long time ago, when I had already the same question to examine, Prof Gray had the kindness to send me the enclosed specimens named, as you see *Phyllocladus aculeatifolius*. (What author) As far as I can judge, this plant is the same as that figured by Goepfert in *Memoir. Des Fossiles Coniferes* as *Phyllocladus rhomboidalis*, Rich. Could you have the kindness to see if ^{Prof} Gray's specimens are not the same ^{species}, or if they are a different species, who is the author of the name. Please excuse me for the question and also please have the kindness to return the specimens. I should write to Prof Gray, to whom please to offer my affectionate regards. But I suppose that he is too busy now and could not attend without trouble to such small matter.

Perhaps you will have time to answer me a short or long, rather very long letter if you would give me great pleasure, especially if it says that you are always quite well, and also heard at work or some new work.

Please also to remember me respectfully to your lady sister and believe me you always gratefully and sincere friend

Any specimens of any kind of *Phyllocladus* would be acceptable.

Geo. Engelmann

Columbus O June 14th 86.

My dear Prof Watson.

Allow me to thank you for the communication of your four memoirs, contribution to American Botany etc. I have received ^{them} with great pleasure as a proof of your kind remembrance. This work of yours, the result of severe studies shows that you are still in good health. You have before you long years of active researches for the advantage of botanical science.

I thought, in the beginning of the spring that I could be able to go East once more and to pay you a short visit. But I am becoming too enfeebled and think I shall do better to stay at home quietly. I have moreover always plenty of materials on fossil plants to work upon, as far as I am able.

Please command me to the kind remembrance of your sister.

Sincerely your old friend
L. S. Desguery

Columbus O. Sept 8th 86.

Answer
J.B.

Prof Sereno Watson Cambridge
My dear Sir

Mass.

Could you give me some information on a book, Paradise Found, published by Mr Warren, President of the University of Boston; informations relating to the Scientific standing of Mr Warren and of the University of Boston. These informations are demanded by Mr. Saporta, a friend, who is interested in the book and wish to know how it is valued or judged in America. As I know neither the book, nor the author, nor even the name of the University of Boston, I should be much obliged for informations on the subject.

You see that I am still living, I am now entering into the old field of vegetable palaeontology. No much to gather there, but I have not to complain as I remain content and well. I hope that you are the same.

Most sincerely yours

Os'o Serqueens

Columbus O Feby. 23^d. 87.

My dear Prof Watson.

You are very well, I hope and still hard at work. You may thank me for not troubling you with my letters. But to day I have to write to you for a subject which does not concern me much, but of which perhaps you have already received letters from Europe. The widow of Prof Tuckerm of Vienna ask me to whom she could apply for procuring the sale of the herbarium of her husband lately deceased. According to what Prof D Ettinghouse and Dr Burgerstein write, the herbarium must be very valuable. Of course, I can only refer the matter to yourself and to Prof Gray supposing moreover that you have received from Mr. Tuckerm some letters like the one I have to day.

Now I wish I could go to Cambridge not merely for the pleasure of seeing yourself and Prof Gray but for a good long look

To some package of the Herbarium, for
really what I have here with me is too in-
sufficient. I would like to see some leaves of the
viscaceous poplars of Ariz. of Fothergilla, and
other foreign Hamamelids, and many many
more. Few of woody plants, out of the American
genre I have little except from Cuba.

Though my health is still good, I can not
go out in cold weather and I doubt if I could
even in the summer be strong enough to have
Columbus. Well! if ever you have some
package of foreign woody plants, no matter how
poor for an herbarian, even ~~one~~ leaves which
you think good only to throw to the dogs,
please think of me and send them the way.

Please to command me to thank and remem-
-ber of Prof Gray and accept for you
the assurance of my highest regard.

Your old friend, sincerely
Geo. Engelman

Columbus 6 July 20th 87.

My dear Prof Watson.

The communication of your beautiful and most valuable memoir on the plants of Jalisco, Mexico is gratefully received, especially as a proof that I still remain in your kind remembrance. I thank you heartily for it. You are quite well, I hope, and perhaps now travelling somewhere in the interest of Science. May this note find you happy, at worst, the best place for naturalists, and strong enough to resist the influence of our boiling temperature.

For myself I remain the same, enjoying the company of my fossil plants and quietly waiting the end of this world's voyage. It has been already very long, as you know.

Always and sincerely your other friend
Geo. Senguer.

Columbus 1 Nov^r 1887.

My dear Prof. Watson:

Two species of *Liquidambar* with borders of leaves entire are known from China and the Sunda Island. It would me of great interest to me to see one or more leaves of one or both of these species. Have you them in the Herbarium? would it be possible for you to send me either one leaf or a whole specimen? which of course I would return to you immediately. In doing it you would really confer to me a great favor.

Perhaps Prof. Gray is back to Cambridge, if that is so, please give him my affectionate regards.

I hope that you are still quite well and working hard and that you have not yet forgotten your old friend
Geo. Engle

Columbus O June 9th 88.

My dear Prof Watson.

You have indeed a peculiar genius for creating fine words, or for transforming into euphonious names such ones which, like my own, are repulsive to ears and eyes by orthography and pronunciation. I could never read *Sesquereunia* without blushing. Now, *Sesquerella* is like one of those delicately flattering ~~Italian~~ Italian diminutives not only acceptable but really lovable. I thank you heartily for this appellation and too for the complimentary remark explaining its right of admittance.

I have also read through-out the arrangement with remarks or descriptions of the species of the genus and feel really obliged for the communication of your memoir, 15th contribution to American Botany.

Very sincerely and respectfully your old friend. Geo Sesquereus

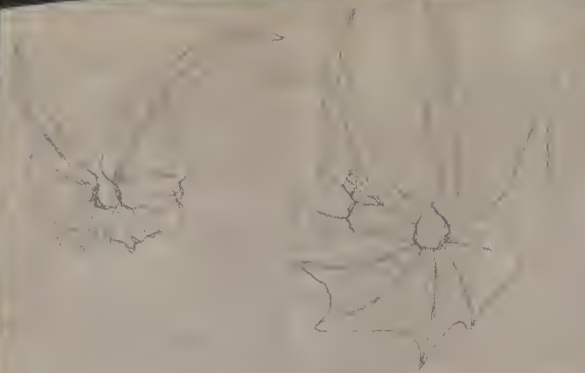
Columbus O Nov^r 12 88.

Très honorée Madame

Daignez me pardonner de vous
écrire ces quelques lignes pour vous accuser
reception du volume "List of the writings
of Dr. Asa Gray" & vous remercier de la
communication que vous voulez bien m'en
faire. Le memorial est fort bien arrangé
& pour moi il est précieux comme exposé
des travaux d'un homme honore' & sincère-
ment regretté de tous ceux qui ont profité
de ses savantes recherches & de son amitié
bienveillante.

Veuillez accepter pour vous même,
madame, l'assurance de mon
respectueux dévouement

Le^r Lesquereux



1. 1/2 in. 1/2 in. 1/2 in.
1/2 in. 1/2 in. 1/2 in.